

SCIENCE

AN ILLUSTRATED JOURNAL

PUBLISHED WEEKLY

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Vérité sans peur.

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FRIDAY, JANUARY 4, 1884.

COMMENT AND CRITICISM.

THE thoughts and actions of young men of intellectual strength, in whom is vested the future fate of scientific progress in this country, are worth attention, and are of the deepest interest to those who are, or soon will be, no longer explorers of new fields. The meeting last week in New York, of the new society of naturalists, composed almost wholly of young men, was remarkable for the force and directness of the discussions, and the absence of pointless and wearisome talk. It became plain that we have men capable of the best work, and that we are preparing for a brilliant future of investigation, whenever the instrumentalities necessary for fullest success are sufficient. The spirit of independence, and the disregard of purely personal influence, were as great as could be desired. All propositions, from whatever source, met with an equal and critical treatment; and no clique or locality had the slightest claim for consideration. Philadelphia was best represented; while there was a striking absence of delegates from Washington, New Haven, and Cambridge.

THE International conference for fixing upon a universal prime meridian and a universal system of time has at length been called by the State department to meet in Washington, Oct. 1, 1884. Diplomatic proceedings are always expected to go on with a certain dignified leisure; but the arrangements for the meeting of this conference have been delayed far beyond any thing customary, even in diplomacy. The act authorizing the conference became a law in August, 1882. As there was

some doubt whether there would be a sufficiently general response to the invitation to insure the success of the conference, a preliminary circular requesting the views of the various governments interested, and an expression of their willingness to enter the conference, was issued from the State department toward the end of 1882. The responses were in some cases favorable, and in others negative or undecided. A desire was felt by the Europeans to have a preliminary discussion of the subject at the International geodetic conference at Rome in October, 1883. The feeling at this conference having shown that there would be little difficulty in the universal adoption of the Greenwich meridian, the final step of calling the conference was taken. Why so late a date was chosen we are not informed.

IN our issue of Dec. 14 we published an article under the title of 'The signal-service and standard time,' criticising the action of the chief signal-officer in not adopting the new standards of time at signal-service stations. We have since learned that our criticism was not well founded, as the information upon which it was based gave an incomplete idea of the position of the service in this matter. It is true that the observers of the service are still governed by the local times of their respective stations; but this is only a temporary arrangement, and will be changed as soon as possible. The reason of the delay is this: the international observation, which is taken at many stations of observation throughout the whole world, is made at seven A.M., Washington time. It is proposed to make this observation eight minutes earlier, or at seven A.M. of the time of the 75th meridian, which is exactly Greenwich noon; but, before this change can be made, the co-operating weather-services and numer-

ous independent observers must first be notified, and their consent obtained. Correspondence has already been begun, and a circular letter sent to all who co-operate in the international work, asking consent to the proposed change. Favorable replies are being received; and there is little doubt that the change will be made, probably Jan. 1, 1885. It should be remembered that the international observation is made largely by observers who kindly co-operate with the chief signal-officer, but who are not under his orders: a change of this kind cannot, therefore, be summarily ordered, but must be made by mutual consent.

It would, of course, be easy to make the change in this country without waiting for the action of observers elsewhere; but this was thought inadvisable. It is a mistake, however, to suppose that the observers are really governed by local times. All observations are made at seven A.M., three P.M., eleven P.M., or other hours of Washington time, and have been so made ever since the establishment of the weather-service. Under Gen. Myers's management, it was thought that it would save confusion at the several stations if the observers kept their clocks at local times instead of Washington time, and observed at the proper corresponding times. This arrangement continues at the present day, though the observations are in reality all made on Washington time. Now, in view of the proposed change in the time of the international observation, it was thought inadvisable to make any change in existing arrangements until the whole change required could be made at one time. The chief signal-officer is in full accord with the reforms in standard time now being introduced, as he has shown in many ways; and he proposes to bring the whole work of the service into conformity with the new system as soon as this can be done without introducing confusion in the different departments of the service.

For more than three hundred years, access to the sacred city of Villa Rica, in Araucania,

has been prevented by the Indians. Its name indicates its importance and wealth in the days of Indian supremacy. Now it is a mere collection of ruins, overgrown with herbage and shrubbery; though the forms of antique monuments and buildings are still traceable, and invaluable for archeological study. Very recently, Chile has taken possession of the territory; and its treasures of antiquity are, or will soon be, accessible to ethnologists.

A SCHEME for conveying brine by pipes from the Cheshire salt-fields to the Mersey, for manufacture there, was started two years ago. The pumping-works are erected, but so far with no results. The scheme was floated on the London exchange; but no 'salt man' joined therein, the general opinion being, that in flowing through pipes for so long a distance the salt would cake, and the stopping-up and corrosion of the pipes would necessitate repairs sufficient to swallow up profits. This would apply to the western New York and Lehigh valley scheme.

It is to be hoped that the state weather-services, of which several are now established, will give attention to questions apart from the ordinary statistical side of meteorological observation, which at present takes so much of their time. Thunder-storms especially need detailed examination from many closely placed observers, such as the state services may possess; for these storms are commonly so small that they often slip, unobserved, through the necessarily coarse meshes of the general signal-service network of stations.

There are as yet, in this country, no observations — at least, none published — of duration or detail sufficient to determine how many hours before its arrival a thunder-storm can be foretold. The antecedent conditions, the area, the average and abnormal tracks, and the duration of these small storms, have yet to be carefully studied. The blowing of the winds about them is imperfectly known. There are no data for determining the relation of the fre-

quency and violence of lightning to the different parts of the storm-area, or for discovering its possible preference for one or another topographical or geological district when it 'strikes.' Some of these points have been studied in Europe, but much remains to be done even there. Indeed, there is no department of meteorology in which local and closely placed observers can attain an end so distinctly original, and so far out of reach of the government service, as in this; and ten years' observations from stations near one another, and numerous enough, would yield results of the greatest practical and theoretical interest.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

Mr. Francis Galton's proposed 'family registers.'

MANY obliging letters reach me from America, offering family information for my use, of the kind described by my friend, Mr. Henry F. Osborn, in your issue No. 39, as that which I want.

The scheme there described is one that I circulated to gather opinions and to obtain guidance before determining its precise form. This is now done, and with your permission I will say a few words upon it.

The information wanted applies to so many different individuals in the same family group, and differs so much in minuteness, according to the degree of kinship, and it has to be arranged in so special a manner, that a copious explanatory description and numerous tables are requisite. There is no real complexity; nevertheless, I feel assured, that, without considerable guidance, endless mistakes will arise. Correspondents will send pages of useless matter; and, on the other hand, they will be silent about simple facts, the absence of which will seriously diminish the value of otherwise copious returns. I therefore found it necessary to prepare a book containing a full account and explanation of what was wanted, in order to exhibit the various hereditary tendencies that converge upon any given person, and containing at the same time all the necessary schedules. This I have done: it is in the press, and will be published about Christmas by Macmillan, and will be procurable in America.

As regards the prize scheme, I found it inadvisable to restrict it to medical men, and I have thrown it open to 'British subjects resident in the United Kingdom.' I could not extend it farther, owing to the extreme difficulty of verifying statements of facts alleged to have occurred abroad. My self-imposed task will be hard enough as it is. The conditions of the prizes are fully explained in a fly-leaf to the English edition.

Let me take this opportunity of saying a few words about another book to which my name is attached as editor, and which will appear at the same time. It is called the 'Life-history album,' and was prepared by a sub-committee, of which I was asked to be chairman, who acted by direction of the Collective investigation committee of the British medical

association. This book gives explanations and schedules for the registration of *personal* data as life advances, just as the *Record* gives for a comprehensive account once for all of *family* data; the details, however, being very different in the two books: they are much more medical in the 'Album.' It is believed by the Life-history sub-committee that the medical value to the possessor, of his own life-history up to date, would be considerable, and of great service to the children. They also feel, that, if these albums are commonly kept, it will be possible hereafter to obtain extracts of a great many of them for purely statistical purposes, which would be of high scientific value. The albums will contain a vast amount of information which is now left to perish, and the lack of which is a great hindrance to obtaining that complete and comprehensive knowledge of the family antecedents of numerous persons, which is at present the paramount desideratum to inquirers into heredity.

I shall be very grateful to any of your readers who may see my forthcoming 'Record of family faculties,' and may make themselves acquainted with what I want, who will send me information concerning their own families. But I cannot explain my wants with sufficient brevity either here or by letter, and must, perforce, refer those who care to know them to the book itself.

FRANCIS GALTON.

42 Rutland Gate, London, December, 1883.

The red sunsets.

I have recently noticed several articles upon the gorgeous sunsets lately seen in this country, and desire to put down a few notes on the same.

The red glare was so brilliant the evening of Nov. 27, that the fire-alarm was sounded in New Haven, Conn., calling out the engines. On the succeeding night the deep red glow was magnificent, appearing far above blocks in the busiest part of the city. Careful observation has shown the phenomenon very nearly as brilliant at sunrise as at sunset. The deep red has appeared the last of all the *colors* in the sky at sunset, and invariably the first in the morning. There has been, in addition to this, a grayish afterglow at night, and in the morning a slight effulgence betokening the rising sun. This afterglow, or effulgence, has made it possible to observe the sky directly at the region where the deep red had just appeared, or was soon to appear; and this invariably showed fine fleecy clouds at a great height, generally stratified horizontally, and extending with slightly increasing density to the south-west or south-east horizon. These light stratified cloud-appearances were visible, even though the sky appeared absolutely cloudless a few minutes before and after the effulgence. The stars the past month have shown, night after night, most extraordinary twinkling, and the air has been saturated with moisture. Again and again, with a high barometer and a perfectly clear sky, sometimes even with a cold north-west wind, I have been astonished to find the relative humidity a hundred per cent.

As to a probable explanation, the wildest theories have been advanced: meteors, cosmic dust, zodiacal light, comets, electricity, volcanic gases and ashes, etc., have each had their adherents. Of these, the last is the only one worthy of consideration. The recent (?) eruptions at Java, 11,000 miles distant, are advanced as a sufficient cause for the presence of the ashes.

That volcanic ashes may be carried great distances is well known. Loomis's 'Meteorology,' p. 77, gives an instance in which ashes were carried 700 miles to the north-east and 1,200 miles to the west of the volcano Coseguina. Notwithstanding this evidence, it

would seem well-nigh incredible that the upper currents and the power of suspension of the ashes could have combined in carrying the particles 11,000 miles.

Common cloud-coloring is caused by diffraction from particles of dust or water-droplets. Light of different wave-lengths has a greater or less power of passing through dust, smoke, water-droplets, ice-spicalae, etc. It is stated that the light at the blue end of the spectrum has less power of penetration than at the red end: hence the light is sifted out, as it were; and the blue disappears first, then the orange, and, last of all, the red (Scott's 'Meteorology,' p. 205). Why may it not be possible that the blue, having the greater refrangibility, is refracted to such an extent as to be intercepted by the earth long before the red has disappeared? Taking into account the great abundance of moisture, the appearance of ice-spicalae (which, however, may have been volcanic ashes), and the fact of the appearance being precisely similar to that ordinarily seen upon clouds, there is no necessity of resorting to the at best doubtful theory of the volcanic origin of the phenomenon.

The similarity between the ordinary sunset and this phenomenon was finely illustrated one evening by a magnificent red-cloud sunset, manifestly caused by clouds comparatively near the observer. These clouds, gradually fading away, were followed by the deeper red so prominently noticed recently, and evidently produced by ice-spicalae at a great distance.

G. A. N.

On the evening of Dec. 22 a red glow was noticed upon the clouds which overspread the whole heaven. On the 23d the cloudiness was complete, and even denser than on the previous evening; but the glow tinged the whole visible vault down to the eastern horizon, and continued for at least an hour after sunset, fading first in the east. On the 24th the clouds were slightly broken. Before 5 P.M. (standard time) a yellowish tinge began to be apparent. At 5.10 the color was reddish, and reached the horizon on all sides. At 5.20 the color was a deeper red, with clouds more broken. At 5.30 the clouds were thin, and showed faint but distinct blood-red color on the eastern horizon, though a little brighter in the west. At 5.40 the cloudiness was reduced to a partial thin film, but a dusky redness was still perceptible in all parts of the sky. At 5.55 the sky was everywhere thinly veiled, but a dark ruddy tint could still be faintly seen all around the horizon. At 6.10 the sky was mostly cloudless, though few stars were visible. A dark-red glow could be discerned in all parts of the heavens, and in the west it rose in broad, ill-defined bands from the position of the sun. At 6.20 no clouds, but only stars of first three or four magnitudes were visible. At first no ruddiness was seen, but shortly it became unmistakably apparent. It was a faint dusky red still obscurely barred in the west. This glow lasted two hours and eight minutes after sunset: atmosphere calm; thermometer sinking from 28° to 25° F. The observations possess interest in connection with similar ones recently made in various parts of the world.

ALEXANDER WINCHELL.

Ann Arbor, Dec. 25, 1883.

Plant distribution in Lower California.

I would call attention to the fact, that many Arizona, New Mexican, and Mexican species of plants, together with more northern species, are found on the narrow strip of tablelands in northern Lower California. Among them I may mention *Quercus Emoryi* and *Q. pungens*, *Astragalus Sonorae*, *Fouquieria splendens*, and many others, with *Geranium*

caespitosum of the Rocky Mountains, *Ivesia Baileyi* of Nevada, *Galium pubens*, *Quercus agrifolia*, the common *Pteris*, *Aquilegia truncata*, and a number of introduced (?) species well known throughout the United States.

CHARLES R. ORCUTT.

San Diego, Cal., Dec. 15.

Kames near Lansing, Mich.

A few years since, I spent one or two days at Mason, some ten miles south of Lansing, Mich. I had hoped to return at some future time, and complete my observations upon some peculiar ridges of sand, gravel, and bowlders in the vicinity of that village; but, as it may be some years before I shall be able to do so, I would like to lay the observations before the readers of *Science*, hoping that some of the Michigan readers may have time to investigate the subject fully.

The surface is here nearly plane. The front moraine of the Saginaw glacier lobe lies some thirty-five miles to the south-south-east, beyond Jackson. These ridges trend towards this moraine from some unknown point north of Mason to another unknown point ten or more miles south-south-east. I was informed that some of these ridges were six and eight miles in length, and are sometimes used as a highway. The drainage is to the northward at present, parallel with the course of the ridges, though I noticed one or two instances where creeks had intersected the ridges instead of being guided by them. The ridges seemed to persist in a northerly course, though with many local exceptions. I noticed one instance in which the main ridge turned nearly at an angle of 100°; but the main course was continued farther north in the heavier ridge, and at the elbow by a much lighter one. The ridges are quite variable in elevation. Perhaps the mean lies between twenty and thirty feet. The slope was not measured, but is, as a rule, too great to permit their being crossed by teams at the natural grade. The component material is all water-worn, and evidently deposited through the agency of water. The bowlders are of all sizes, up to twelve inches. Perhaps forty per cent were sandstone, similar in lithological characters to the subjacent rock strata. The remainder were metamorphic or igneous species, except some limestone pebbles.

Whether these ridges were formed in the longitudinal crevasses and river-channels of the ancient glacier, or not, must be determined by a more careful survey of the region than the writer was able to make in the few days spent at Mason.

L. C. WOOSTER.

Eureka, Kan., Dec. 17, 1883.

Longevity in a fasting spider.

On the fifteenth day of October, 1881, I enclosed a spider in a small paper box. From that day to the seventh day of May, 1882 (204 days), I carefully watched and daily inspected the prisoner, and can positively affirm that he partook of no food or water. The box in which he was confined was as clean and white as white paper could make it, and remained so while he continued to occupy it, except for the appearance of a few dark specks which I suppose to be the droppings of the prisoner. I carefully observed him every day, and sometimes two or three times in a day; and I was unable to detect any emaciation or symptoms of weakness, or even irritability of temper, while he lived. He always appeared as active, and looked as plump and healthy, as he did the day I dropped him into the box, until within three days of his death, when I first observed that when the box was tipped he would fall from his position.

WILLIAM JONES, M.D.

Newburgh, N.Y.

The pedunculated lateral-line organs of *Gastrostomus*.

The recent discovery of a form of deep-sea fishes closely allied to the Eurypharynx described by M. Vaillant, by the U. S. fish-commission steamer Albatross, has afforded excellent opportunities for a more thorough examination of the external characters presented by the skin of these forms. This species of eurypharyngoid fishes, — the one studied by Professor Theodore Gill and myself, and named by us *Gastrostomus Bairdii*, — upon closer examination of the region of the lateral line, discloses features which appear to be somewhat remarkable, if not unique, amongst organs of the kind hitherto known.

The lateral line is in its usual position, and begins just behind the head. There is no mucous canal covering the end-organs; but these are isolated in groups of from two to five, standing on the skin in an oblique row at the hind margin of each muscular somite. The groups consist, in fact, of from two to five stalked organs, as shown in fig. 1 in the cut. The stalks are not pigmented at all, except at the tips, where they support a discoidal cup-shaped organ, which is more or less completely pigmented internally. In some instances these end-organs are very distinctly cup-shaped; in others that form is less clearly apparent. The base from which the stalks arise is not so deeply pigmented as the surrounding skin, which is very densely loaded with pigment, and very black. The pigment on the basal disks is, in fact, arranged in a slightly reticular manner: the pigmented layer is continuous with the outer clear sheaths of the stalks; and the medullary portion of the stalk can be seen in some cases to consist mainly of nerve-fibrils, which pass outwards to the cup-like organs at the tip. In a few cases there appears to be a clear space in the centre of the cup-like end-organ, as shown in fig. 2, surrounded by a dense circle of pigmented tissue.

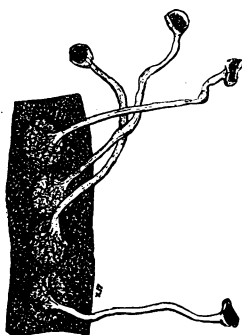


FIG. 1.



FIG. 2.

The function of these side-organs of *Gastrostomus* is apparently tactile, or may serve a special purpose at the great depth in which this fish lives. They remind one very forcibly of the rows of comb-like end-organs which have recently been described by F. Leydig on the head of the cave-fish (*Amblyopsis spelaeus* DeK.); but in this case the stalks are not so robust, and are much more slender, and relatively longer. It may even be that these lateral bands of side-organs of *Gastrostomus* are phosphorescent at their tips, like the side-organs of scopolids, steroptichids, etc. The lateral bands made up of short oblique rows of these organs, as the fish moves through the water at a depth of five to fifteen hundred fathoms, may possibly become luminous.

That they are also sensory in function there can be no doubt, being found in the usual position of the lateral line, as in common fishes, and, like it, probably innervated from the vagus. The stalks are fully a sixteenth of an inch long, and are apparent

on the side when the fish is immersed in alcohol or water, and project outwards quite freely, so as to be visible along the sides when the fish is viewed from above. These naked side-organs remind one also somewhat of the naked nerve-hills on the sides of the body of young fishes, such as those of *Gadus* and *Gambusia*. In the former the stiff sensory hairs of the nerve-hills project immediately from the surface of the hill into the surrounding water, but in no embryo fishes am I aware that the side-organs are ever pedunculated. In fact, the side-organs of *Gastrostomus Bairdii*, like the whole of the rest of the organization of the animal, particularly its skull and branchial apparatus, present an extreme phase of specialization.

J. A. RYDER.

RETROSPECT AND PROSPECT.

WITH the present number *Science* enters upon the second year of its existence. The time is an appropriate one, while extending a cordial greeting to its readers, to call their attention to its work and its purposes. That a journal of popular science, with the varied and informal contents appropriate to a weekly publication, would, if judiciously conducted, prove a welcome addition to the list of American periodicals, has long been felt by those most interested in scientific progress; but, when the numberless difficulties in the way of success had to be considered in detail, they were found to be numerous and perplexing. The general scope of the journal was the only feature about which little doubt could be felt. Two quite distinct yet inseparable objects of existence presented themselves: one was to keep the readers of the journal informed of the progress of science in all its branches; the other, to give expression to the well-matured views of scientific men upon all public questions connected with the increase of knowledge, and thus to become, so far as possible, an organ of public opinion upon scientific affairs.

In pursuing the latter object the path of duty was too plain to require discussion. The journal must be the organ of no individual, clique, or party, but must, while preserving entire impartiality, give plain and fearless expression to its convictions upon any question in which the interests of science at large were involved. How far it has fulfilled this requirement is a question to be decided by its readers and patrons, without argument from ourselves.

The question of the contents of the journal in detail was a far more intricate one. Shall its articles be designed exclusively for the specialist, or shall the results it makes known be popularized by the omission of all purely technical nomenclature? Shall they be long and elaborate, or short at the risk of incompleteness? Shall they be strictly and purely scien-

tific, or shall the speculative, sentimental, and poetic sides of things be allowed to appear? Shall its chronicles of progress consist of the briefest possible memoranda of all important current researches, each duly labelled for reference, or shall a selection be so made that each account shall be prepared with a statement of the origin, place, and object of the research, with a view of making its true significance known? In the case of scientific articles, where shall we draw the line between what belongs to this journal and what to those intended for the publication of original researches? These are merely a few of the more important questions which the projectors were obliged to meet, and which they have endeavored to decide in the way best fitted to give general satisfaction. The result is seen to a certain extent in the present number; but some aspects of the subject may be profitably considered from a broader field of view.

The difficulty arising from the technical nature of scientific researches admits of being partially resolved, so far at least as the general principle is concerned, by a very obvious consideration. Science must be almost as much popularized, to be made accessible to all scientific readers, as to be readable by the educated public who were never in a laboratory. A new formula in thermodynamics is as incomprehensible to a botanist as to a member of Congress. The average physicist knows as little about a brachiopod as the average merchant. What the most modest well-read clergyman may fairly think he knows about Darwinism far exceeds all that the common run of chemists really do know. The obvious conclusion is, that, should we seek to make discussions of current scientific researches accessible to all scientific readers, we cannot avoid being somewhat popular in style.

On the other hand, if the journal should present to its readers only that class of reading-matter which they get for nothing in the daily papers, its very existence would be a superfluity. To justify the publication of any periodical devoted to a specialty, it must present its readers with a kind of matter which they cannot find in the public prints.

The term 'popular science' is often made to include a class of discussions quite different from the presentation of scientific truths in common language. Science at the present day is the ideal of democracy. Its work and its honors, from the highest to the lowest, are thrown open, without restriction, to all men. There is no authority which can say to the humblest worker, "I know this, and you do

not: I am therefore above your criticism, and you must accept my statements without essaying to inquire into the validity of their foundation or the soundness of their application." There is no tribunal in the scientific world which has the power to proclaim what is and what is not proved; what problems are and what are not solved. To one who has never considered this state of things, the first impression felt is, that it must imply universal anarchy; that in a community where every one has equal authority—that is, no authority at all—there can be no such thing as permanent and widely received opinions. But the very opposite is the truth. A system which requires every doctrine to stand on its own merits, and to maintain itself only by being proof against every assault, is the very one under which truth stands the best chance of showing its permanency. A long-established scientific doctrine stands like the Matterhorn, not through being protected from assault, but by being able to resist the storms of ages.

Now, there is in every civilized country a class of writers who avail themselves of this principle of equality to discuss subjects of which they have no accurate knowledge, to propound new theories, and to attack old ones. A voluminous literature thus arises which is the work of the lay element in the scientific community, and which is therefore sometimes called popular science. Such productions must stand on their merits as much as the propositions of the professional scientific man, and are entitled to consideration only according to their merit. The policy of *Science* is to admit nothing to its pages which does not belong to the domain of knowledge, excluding with especial care speculations upon subjects like the nebular hypothesis in which many active minds are so fond of indulging.

After a careful consideration of the form in which the results of current researches should be presented, it has been decided to substitute for the weekly summary heretofore presented brief discussions of current work which shall be of more interest to the general reader. To combine brevity with perspicuity in such cases is often a very difficult problem, in which the golden mean affords the only solution.

The form in which *Science* is now presented has been to a large extent the result of careful inquiry among its accessible friends and patrons. Manifestly, the plan in view cannot be developed in a single number; but we hope that a few weeks will show our purpose to make *Science* of greater value than heretofore to our widening circle of readers.

*THE MARINE LABORATORY OF THE
JOHNS HOPKINS UNIVERSITY.*

THE Chesapeake zoölogical laboratory was instituted by the trustees of the Johns Hopkins university as part of the biological department of that university in 1878, and Dr. W. K. Brooks was appointed director. Its purpose is twofold, — to furnish complete facilities for original studies in marine zoölogy, and a place for more elementary instruction. The fauna of the southern waters of the United States was selected for study. In providing thus a place

Topsail Inlet, ten miles west from Cape Look-out, protected from the ocean, except in its worst moods, by a broad sand-bar, and yet so near that an hour's sail carries one out upon the high seas. Owing to the configuration of the coast-line, the warm Florida current flows by and almost bathes the shore. This warm current, setting up from the shores of the Gulf, sweeps along with it many pelagic animals which belong to a hotter climate. Yet, while the ocean-life is decidedly southern, the climate of Beaufort is not oppressive: indeed, the place and its neighbor, Morehead City, are summer



MARINE LABORATORY AT BEAUFORT.

for advanced work, this university has taken the initiative among American colleges; the various summer schools held along our coast being more particularly concerned in instruction than in investigation of new problems. The first and second sessions in 1878 and 1879 were held in the lower parts of the Chesapeake Bay. In 1880 the laboratory was moved to Beaufort, N.C.

Beaufort has been a favorite haunt of naturalists ever since 1860, when it was visited by Drs. Stimpson and Gill. No better place could be selected for the study of the forms of life in southern waters. It lies at the mouth of Old

resorts. The town, standing almost in the ocean, is swept by nearly constant breezes, which temper the heats of July and August.

The place is quite accessible, being only two miles from Morehead City, the eastern terminus of the North Carolina midland railway, and may be reached by steamer from Norfolk *via* Newberne, and by rail from points north and west *via* Goldsboro.

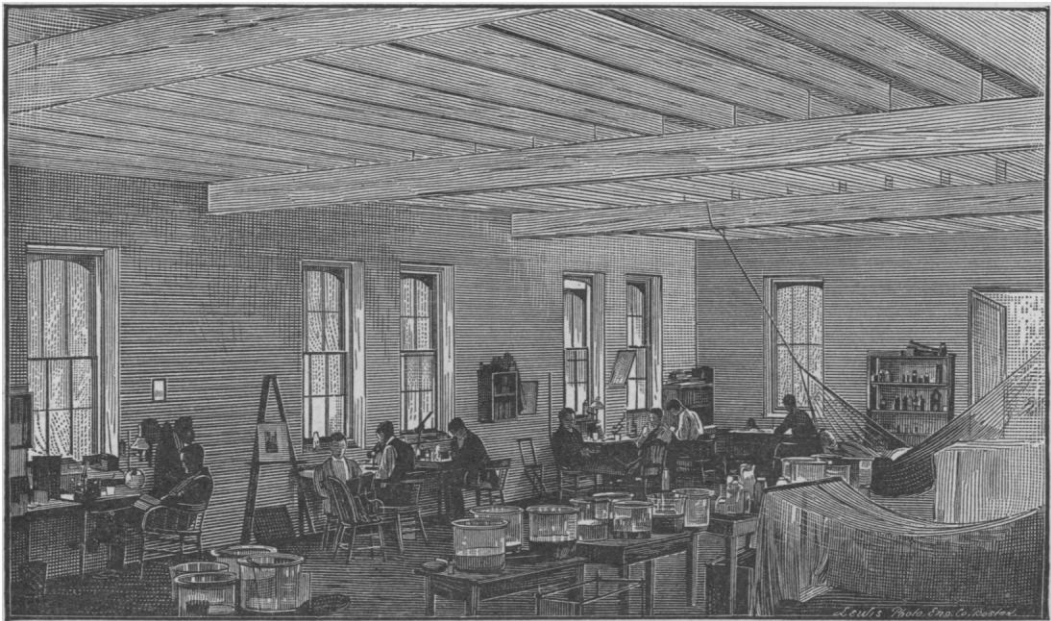
The site of the laboratory at Beaufort is most convenient, being at the very water's edge. A pier built out from the front gate to the deep water crosses a flat of black soft mud, bare at every low tide, and a place where the

specimen hunter is richly rewarded. A search here obtains for one crabs and hermits, *Porcellana* in tubes with *Chaetopterus*, *Alpheus*, annelids, mollusks, echinoderms, ascidians, and barnacles upon the wharf-piles. The general student can here find material to illustrate his study of almost any of the larger groups literally within a stone's throw of his work-room. From the end of the wharf at high water the dip-net secures not only quantities of things to interest the general student, but crustacea, medusae, *Sagitta*, and larvae of the greatest interest to the specialist.

Across the channel which runs along the

Trawling in the sound procures starfish, echiroids and ophiurans, *Chiton*, *Fissurella*, *Leptogorgia*, *Astrangia*, often with large masses of coral. High tides sweep in pteropods, *Sagitta*, *Leucifer*, *Siphonophora*, pelagic larvae, and medusae of great interest, such as *Liriope* and *Cunina*. The rocks upon the artificial break-water furnish *Penophora*, tubularian hydroids, and several species of actinians. On shells in-shore are found the known genera of entoproctan Bryozoa.

But I cannot give a complete list of the fauna here, nor even mention all the attractions. I have not tried to do so, but merely to inti-



INTERIOR OF MARINE LABORATORY AT BEAUFORT.

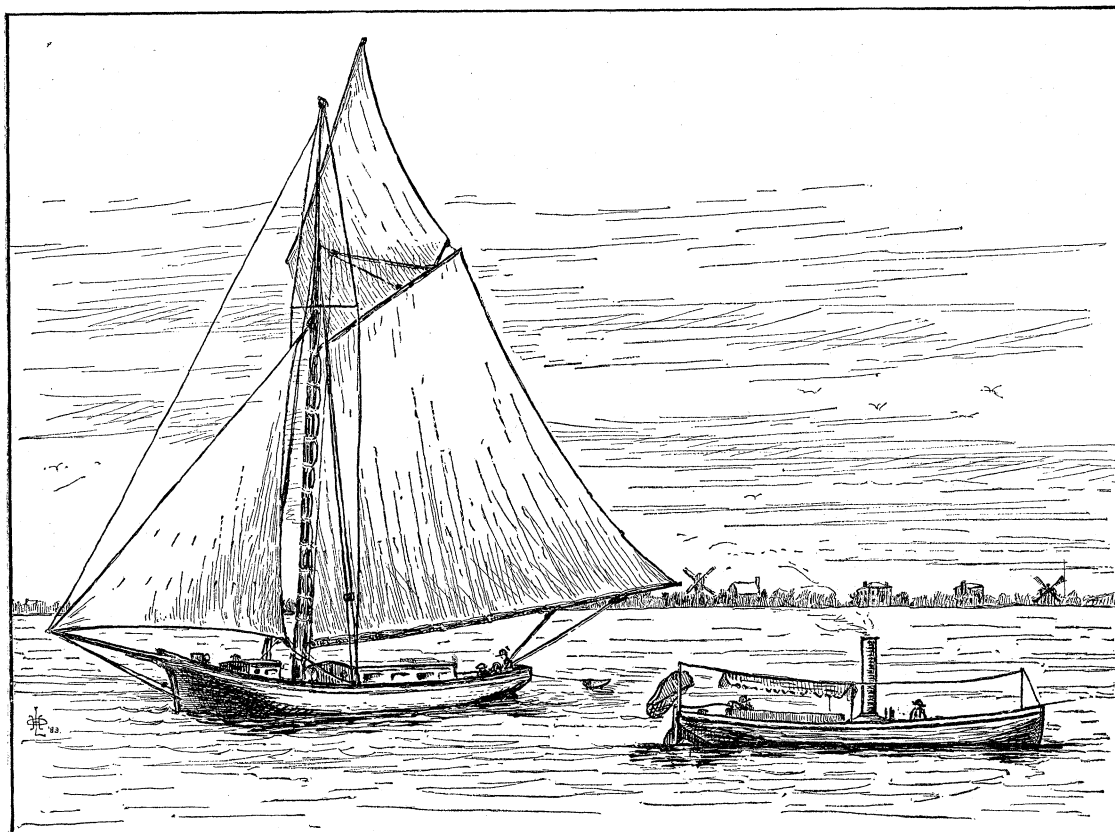
water-front is a large sand-shoal, uncovered during several hours every day; and here are the favorite haunts of myriads of interesting creatures. I say myriads advisedly, for one of the most striking features of the Beaufort fauna is the extreme abundance of almost all the forms which occur there at all. The inner side of this shoal is literally honeycombed by a colossal species of *Balanoglossus* often three feet long; and on the outer edge are to be found *Mellita* in great numbers, and dead shells of *Mellita* inhabited by *Thallasema*, as many as one has the patience to collect. All over the shoal creep *Limulus*. In the deeper water just off the shoal are *Renillas* without limit and the beautiful nudibranch *Pleurophyllidia*.

mate the exceeding variety and abundance of forms of the greatest interest. Though Beaufort has been the resort of naturalists for the last twenty-five years, it has not yet been to any degree exhausted.

Since Beaufort was felt to be a somewhat transient location for the laboratory, a permanent building, with all the modern conveniences for work, was not erected. A two-storied double house, with eight rooms, was rented for work-room; and houses adjoining were secured as living-quarters for the party. Thus, both in their work and in the life out of the shop, the party was kept pretty well together; and the members had that opportunity of forming personal acquaintance with one another, the

value of which does not require comment. The furnishing of the building was simple, — tiled tables, with lights only; and other luxuries were dispensed with. Not even were pumps erected to maintain a constantly renewed stream of salt water circulating through the aquaria. In their place was used the cheaper and very effective device of aeration by means of a stream of fresh air constantly forced through the aquaria by a Sprengel pump.

and with a draught of about thirty inches, capable of taking us to any points about the sounds and rivers, or even of venturing out to sea when Old Prob did not menace with danger-signals. The launch was most useful; and was in service almost every day for dredging, trawling, or carrying parties out to tow in the open ocean. She is, however, but a passing stage, as her name *Nauplius* implies; and we hope some day to possess a full-grown steam-



YACHT AND STEAM-LAUNCH OF JOHNS HOPKINS MARINE LABORATORY.

Facilities for work were not, however, in the least curtailed; and all the apparatus for capture and means for getting about were provided. The dredge and trawl, spade and sieves for bottom fauna, towing and dipping nets of silk bolting-cloth for surface forms, and many special traps devised for the capture of particular animals, formed a complete array of appliances. Besides its small boats, the laboratory has for several years possessed a steam-launch of Herreschoff pattern, twenty-seven feet long,

vessel, in which we can with safety explore the deeper waters offshore, which are as yet almost entirely unstudied.

During the past summer the navy has received an important addition in the form of a yacht, to be called the *Zoea*, though at present otherwise registered. She is a full-rigged sloop, forty-seven feet long, fifteen feet beam. She won a silver cup in a regatta upon the Potomac while in her former service, and her speed was a pleasant feature of collecting-trips

in her. Her sailing qualities do not at all unfit her for our work. Her cabin has ample accommodations for four persons, and could stow eight; and the cuddy forward has room and all the utensils for the cook: so that cruises to a distant dredging-ground can be undertaken without inconvenience, by a fair-sized party.

Of the usefulness of the Chesapeake zoölogical laboratory we may feel assured, though it is still in its infancy. It has held six sessions. During that time there has been a total attendance of fifty, of whom fourteen have been in attendance at least two sessions. These fifty men have been gathered from more than twelve different colleges, and are at present located in fourteen different states, besides two who came from Canada, one from Cambridge, Eng., and one from Japan.

In 1879 the laboratory was in co-operation with the Maryland fish-commission; and Dr. Brooks devoted most of his own time during the season to a study of the oyster, with especial reference to its embryology and its artificial propagation. The theoretical results of his work are of the greatest significance; but he succeeded in artificially fertilizing the oyster's eggs, and shedding such light upon the habits of reproduction that the greatest interest was aroused, and zeal in the search for some practicable method of oyster-culture, to replenish the waning oyster-beds. This interest has resulted in the discovery of a practicable method.

I will not recapitulate all the scientific papers published as resulting from work done in the laboratory: suffice it to say, that important memoirs have been published upon *Lingula*, *Squilla*, *Leucifer*, *Renilla* (the last two being published in the *Philosophical transactions* of the Royal society), *Thallasema*, and a monograph, not yet complete, of the *Hydromedusae* of the south coast. Beside these memoirs, the various members of the laboratory have written numerous shorter papers, which have been published in the *Quarterly journal of microscopical science*, the *university Studies*, and *Carus's Zoologischer anzeiger*. These articles, embodying the results of the laboratory's work, number, in all, fifty-nine separate titles.

For the most part, the laboratory has been morphological in the aspect of its work; not exclusively so, however, for both in 1881 and 1883 Dr. Sewall worked there upon selachians with reference to the equilibrium-sense function of the semicircular canals.

Last summer (1883), after three years at Beaufort, the laboratory was moved back to the Chesapeake Bay, and located in a building

rented from the Hampton normal school. The location was in many respects not a good one, for it was far away from the best collecting-grounds and supplies of pure salt water; but it was selected to permit the laboratory to co-operate with the Maryland state oyster commission in experiments upon artificial propagation, and other expedients for a rapid and reliable method of restocking the oyster-beds in Chesapeake Bay. Lieut. Winslow, U.S.N., detailed for special service, was with us during most of the summer; and in the early part of the season the oyster-police boat, Gov. Hamilton, was stationed at the lower end of the bay.

The results of the season's work are not yet so far worked up as to permit one to speak about them. We had among us Mr. William Bateson of Cambridge, Eng., who came over to work upon *Balanoglossus*. His work includes a more thorough knowledge of the larval history of *Balanoglossus* than has been hitherto attained, and promises much that will be of greatest interest in respect to that most problematical creature. HENRY L. OSBORN.

THE DETERMINATION OF THE OHM.

THE importance of having a uniform standard of electrical resistance is so apparent, that the establishment of a unit which shall be suitable for practical work, and will also satisfy the demands of electrical science, has for a number of years been regarded by all electricians as of the first importance.

The requirements of such a standard are, that it shall be easily reproduced or verified; that it shall have a simple relation to the units of work, heat, etc., and therefore be based on the fundamental units of length and time; and, finally, that it shall be of so great resistance as to be suitable for all ordinary practical work.

In the year 1862 the British association decided that a unit of resistance based simply on the earth quadrant, or ten million metres, as the unit of length, and the second as the unit of time, would be of such a magnitude as to best satisfy the requirements of the case. Experiments were then undertaken by a committee of the British association with a view to the construction of standards which should accurately represent this unit of resistance, or *ohm* as it was called. Owing to some minor defects in experimentation, and to an unaccountable error in the determination of the coefficient of self-induction of the revolving coil, their result was in error. This standard British association unit, as it is now called, is confessedly too small; but it is the basis of the

so-called ohm-coils that are in current use. The latest experiments indicate that the value of the British association unit is .9867 ohms; this result having been obtained by Lord Rayleigh by two distinct methods, and by Mr. Glazebrook by still another method. But different observers still differ quite widely in their results.

The International committee on electrical units, which met in November, 1882, in Paris, in view of the present unsettled state of the case, and the necessity for the speedy adoption of a suitable standard, decided that when the length of a column of pure mercury of one square millimetre section, and having a resistance of one ohm, shall have been determined to within one part in a thousand, the ohm shall then be defined as the resistance of such a column of pure mercury of the determined length; and the different governments represented were urged to prosecute experiments for the accurate determination of this length. For this purpose, among others, an appropriation of twelve thousand five hundred dollars was made by the last Congress of the United States. The work on the unit of resistance is under the charge of Professor Rowland of the Johns Hopkins university; and the experiments are being carried on in Baltimore, both at the university and at Clifton Park, two miles from the city. Owing to some unexpected delays in the construction of necessary apparatus, the work that has been undertaken first is the determination of the specific resistance of mercury in British association units. This has been experimented upon by measuring the resistance of columns of pure mercury contained in glass tubes of various calibers and lengths, so that the resistances of the columns experimented upon range from one to ten British association units. The remaining part of the work is the determination in ohms of the resistance of the British association standard used in this determination of the specific resistance of mercury. Two principal methods will be employed for this purpose.

First, the resistance will be found by means of the mechanical equivalent of heat. The apparatus used by Professor Rowland, in his well-known work on that subject, has been set up for this purpose. It is proposed to heat some non-conducting liquid, such as alcohol or turpentine, by means of the heat developed by the passage of the current of electricity in a conductor whose extremities are kept at a known difference of potential. The same heating will then be produced, under the same circumstances, by purely mechanical means; and the

resistance of the conductor will thus be determined directly from the work-equivalent of the heat developed in the conductor.

The second method to be used is that of Kirchhoff, as modified by Rowland in his determination of the ohm in 1876. The instruments will, however, be in large part new, and constructed expressly for this research; so that a new set of instrumental constants will be involved. A third method, the earth-inductor method of Weber, will also be used if time permits.

For these experiments it is proposed to use, as a source of electricity in the calorimetric method, fifty Planté cells charged by a small dynamo machine. For measuring large currents of electricity an electro-dynamometer has been constructed, with the Helmholtz arrangement of two large coils and a single small suspended coil. The diameter of the large coils is about one metre: that of the small suspended coil is about twenty-five centimetres. There are two sets of large coils, — one wound with large wire, about no. 8; and the other with much smaller, about no. 15. There are also two small suspended coils wound to correspond. This arrangement gives the instrument great power and range. The divided circle was made by Fauth & Co. expressly for this instrument. Four induction-coils are to be wound in four parallel equidistant grooves, turned on the outside of a brass cylinder about one metre in diameter. These coils will each consist of about two hundred turns of no. 15 copper wire. This arrangement will afford great variety in the manner in which the several coils may be combined; for the inductive action of each coil upon each of the others may be taken, giving three simple combinations for each coil.

The trustees of the Johns Hopkins university have kindly placed the Clifton House at Professor Rowland's disposal for the conduct of these experiments; and, as it stands in extensive grounds at a considerable distance from the road, it will be peculiarly suitable for delicate electrical experiments. Piers have been built for the different instruments, and a small steam-engine set up for supplying the power necessary for running the dynamo machine and the mechanical equivalent of heat apparatus. The actual experimentation will be carried on, under Professor Rowland's direction, by A. L. Kimball, assisted by H. R. Goodnow and Ensign Louis Duncan, U.S.N.; the latter having been specially detailed for the work by the Navy department.

It is hoped that a satisfactory conclusion will be reached by September, 1884.

PECULIARITIES OF WEATHERING IN THE POTTSVILLE CONGLOMERATE.

THE striking characteristics of the Pottsville conglomerate in eastern Pennsylvania are its highly siliceous composition and its solidity. Owing to a consequent great durability, it stands out prominently along the

stratification are found in an apparently homogeneous rock. In such cases a difference or deficiency of cementing-material must be the directing cause.

Weathering action across the plane of stratification is exhibited in its first stages by shallow and narrow grooves, which run sinuously across the rock. These have their origin in

little streams of rain-water which flow from the surface down the sides of the rock. Once started, such a groove forms a channel whose drainage capacity constantly increases as the depression enlarges; and by degrees the fine groove grows to a decided fissure, half a foot or more across, which the continued action of rain-water cuts deeper and deeper into the rock. This fissure is generally of approximately uniform breadth; but, as it enters

farther into the rock, the water drains into it from all sides, and an enlargement is sometimes formed at the end, which I have seen to result in an almost circular hole, completely penetrating the rock.

The most peculiar and remarkable of all the results of this weathering action are, however,

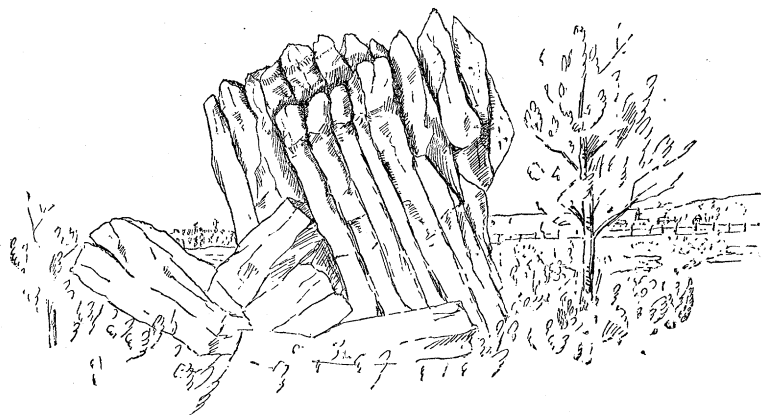


FIG. 1.—Outcrop showing weathering along the plane of stratification.

different mountain ridges which surround the anthracite coal-basins; but though, as compared with the associated rocks, its resistance to weathering is very great, the effects of this action are everywhere revealed on examination.

The surfaces of the finer and more compact varieties are frequently seen to be covered with numerous small holes, or pit-marks, resulting from the removal of separate grains. Blocks of the coarse pudding-stone have generally a very rough surface, the pebbles projecting half their thicknesses above the surrounding matrix; and fragments of this rock are sometimes so thoroughly permeated and softened by percolating water that they can be crushed to grains by the hand.

Along the planes of stratification the sub-aerial decay of this rock is particularly well marked. Deep clefts and gashes are found along these planes, which frequently cut entirely across large masses, dividing them into separate slabs. This action is best developed along the upturned edges of steeply inclined dips, where water has the best opportunity to accumulate and to prolong its action in incipient grooves; and, with isolated blocks only slightly inclined, the increased decay along the upturned edges, due to this same cause, is often noticeable. A somewhat remarkable fact about such weathering is, that clefts parallel to the

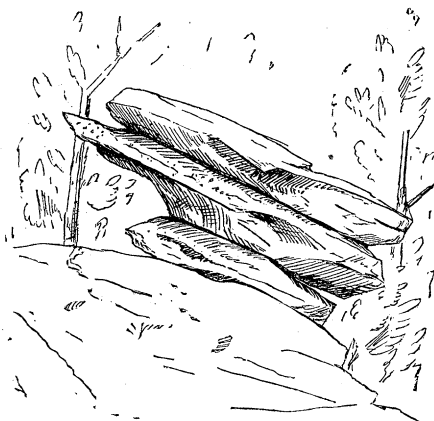


FIG. 2.—Isolated conglomerate mass showing increase of weathering along the planes of stratification on the upturned edge.

those produced by a superficial action in the plane of stratification. Over flat surfaces of the rock, white, washed-looking patches occur; but where a slight depression exists, the water

accumulates and stands, and as a consequence the grains of the rock in immediate contact are loosened, and, on the evaporation of the water, blown away.

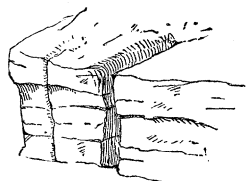


FIG. 3.—Weathering across the plane of stratification.

Thus the depressions which were at first, perhaps, only a fraction of an inch, are deepened, and, by degrees, basins of as much as a foot in depth are eaten out. These are often so regular in outline, and with such smooth sides, that they might readily be mistaken for pot-holes; and, indeed, it was such that I first considered them, and was puzzled to account for the peculiar channel in which the waters producing them must have flown. A distinguishing feature of these depressions, however, is that each one has an outlet cut down

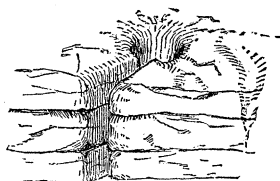


FIG. 4.—Enlargement at end of fissure.

to near the bottom of the cavity; and this is easily accounted for, on the theory of their subaerial origin, by considering, that, once such a basin started, the overflow would always pass off over the lowest edge, and as the basin increased in depth, by continued dissolving action, so would the outlet also. A further confirmation of this is furnished by the facts, that in inclined rocks the outlet is always towards the lower rim, and the bottom of these cavities is either horizontal or sloping towards the outlet. In the bottom is also generally accumulated a small amount of gravel and sand recently loosened from the bed. These basins are of all sizes, up to three feet

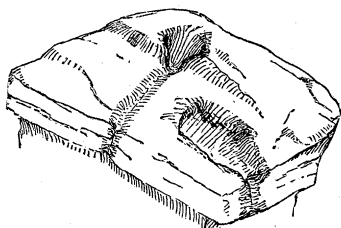


FIG. 5.—The results of superficial weathering in the plane of stratification.

and more in diameter. Their shapes are varied, — sometimes circular, sometimes oblong, — with gently sloping sides, or steep, even re-

curving ones, according to the character of the rock. They are frequently connected in strings by narrow channels, like a miniature lake system; and, with the enlargement of these channels, a simple, deep groove across the rock results, all this action combining to give the rock a very rugged appearance.

The very preponderance of silica grains in this rock, to the exclusion of any good cementing-material, is probably one of the chief reasons for its decay. Rain-water is, without doubt, one of the most active agents; but the secretions from the thick growth of moss and lichens, which frequently covers the surface and penetrates into the cavities of the rock, have probably also their effects. The deep gashes produced by the action of the rain-water offer excellent opportunities for frost to continue the work of destruction; the ice forming in these clefts, and, by its prying action, completing the separation of the already partially divided mass.

As a consequence of this wide-spread weathering process, large continuous outcrops are rarely found. Collections of huge blocks generally mark their site; and the thick accumulations of smaller fragments, which are so frequently found over conglomerate areas,



FIG. 6.—Large basin in conglomerate, with a double outlet.]

result, without doubt, from the further subdivision of these larger blocks.

The products of decay either accumulate in place, are washed down by streams, or blown away by the wind. On the top of Broad Mountain, and elsewhere, the disintegration *in situ*, I am informed, is so great that the loose rock is dug out as gravel; and, in valleys watered by streams flowing down from conglomerate ridges, deep deposits of siliceous sand are found, valuable for building-purposes.

The decay of the sandstones and shales, associated with or underlying the conglomerate, is even more pronounced than in that rock. Changes of color, especially from the greenish tints to red, brown, and yellow, are the most

frequent results; and this is often accompanied by a softening to a barely coherent sand or clay. Erroneous conclusions are thus frequently drawn from surface indications, as to the nature of the underlying rock.

The subject of the decay of rocks has re-

Smith, J. A. Tanner, M.D., and H. W. Eaton, Ph.D., Louisville—was not appointed till about three weeks before the close of the exposition: hence thorough tests were impossible.

As the U. S. company did not enter into the contest, there was no competition on the

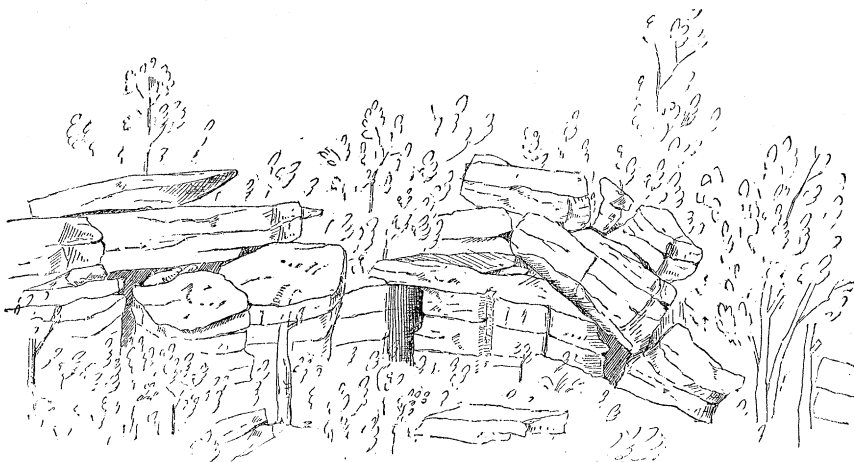


FIG. 7.—Broken conglomerate outcrop.

cently been admirably treated by Dr. T. Sterry Hunt,¹ chiefly with regard to the crystalline rocks; and it deserves to be further studied, in the case of these more recent rocks, from its evident importance in chemical geology, its interesting and well-known relation to topography, and its economic bearing. ARTHUR WINSLOW.

Pennsylvania geological survey.

ELECTRIC LIGHT TESTS AT THE LOUISVILLE EXPOSITION.

THE display of electric lights at the Louisville exposition, as to number, was the greatest ever made in and around one building. The number of lights used varied somewhat, but the average was about as follows:—

	Incandescent lights.	Arc-lights.
Edison isolated lighting company . .	4,600	—
U. S. electric light company	210	29
Fort Wayne Jenney electric light company	—	100
Thomson Houston electric light company	—	36

The jury—consisting of Benjamin Rankin, Louisville; W. W. Weaver, Chicago; Charles

¹ The decay of rocks geologically considered. By T. Sterry Hunt, LL.D., F.R.S. *American journal of science*, September, 1883.

incandescent lights. However, the following tests were made: connection was made with a circuit containing 315 lights at what was considered an average point in the circuit; and fifteen lamps, five of them new and the balance selected systematically from the circuit while lighted, were tested in a specially constructed photometer-room while indicator-cards were being taken from the engine.

A Bunsen photometer with a twelve-foot bar was used, and the horizontal intensity determined with the carbon at an angle of 45°. The intensity of the (nominally) 16-candle lights varied from 12 to 19.66 candles, averaging 13.77 candles; and the average horsepower was 32.50. These figures give 9.70 lights, or 133.57 candles, per mechanical horsepower.

The action of the automatic regulator was then tested with a light in the photometer, first 50 and then 100 lights being thrown off and on. In one of the six cases the variation was 1.23 candles, but in all the others it was less than .66 of a candle. Only a momentary flicker was noticed as the lights were thrown off and on.

The jury reported as follows: "The tests of the Edison system are most satisfactory as to the efficiency of the various appliances, the steadiness of the light produced, and the general results. It is a matter worthy of note, that

during the 100 days of the exposition, with over 4,000 lights burning, there was not at any time a suspension of light from failure of the appliances of the Edison electric lighting company."

Of the arc-lights, lamps were chosen, one at a time, from the circuits, and inserted in the same circuit in the photometer-room, care being taken that no change was made in the circuit adjustments. Indicator-cards were taken from the engine used, during the testing of each lamp. The strength of current, and fall in electromotive force, were also determined with an amperemeter and voltmeter; but, as only relative results were desired, these instruments were not graduated.

The photometer-bar was fifty feet long; and tests showed that there was no reflection vitiating the results, from the dead-black surface of the walls of the room. The photometric tests were made with an Edison incandescent light as a standard. Fifteen tests from candle to incandescent, and ten from incandescent to arc lights, were made for each lamp, five arc-light tests being between the same number of tests of the standard.

The arc-light was cut out during the tests of the standard, and a new cup was allowed to form before the next set of tests was made.

The dynamos were worked to their full advertised capacity in regard to the number of lights in the circuits; and four lights were tested in each case, with the following results:—

	Jenney.	Thomson Houston.
Total number of lights in circuit	16	12
Total mechanical horse-power	26.92	11.79
Average horizontal intensity in candles . .	496.5	291.8
Average intensity per horse-power	306.5	296.9
Relative efficiency of lamps from light, current, and fall in electromotive force .	1.055	1

From these tests, and an examination of the dynamos, lamps, regulators, etc., the awards were made as follows: to the Edison company, for isolated lighting, medals for the best incandescent system and light, and for the best dynamo and lamp for the incandescent light; to the Fort Wayne Jenney electric lighting company, medals for the best system and dynamo for arc-lighting; but, to the Thomson Houston electric lighting company, a medal for the best arc-light, because, "while the light of the Jenney was slightly stronger per horse-power of electrical energy used in the lamp, it was not quite so steady as the Thomson Houston." H. W. EATON.

THE LATE MR. DARWIN ON INSTINCT.¹

AT the meeting of the Linnean society this evening (Dec. 6) a highly interesting posthumous paper on Instinct, by Charles Darwin, will be read and discussed. We have been favored with an early abstract of the same, which we here present to our readers.

After detailing sundry facts with reference to the migratory instincts of different animals, Mr. Darwin proceeds to suggest a theory to account for them. This theory is precisely the same as that which was subsequently and independently enunciated by Mr. Wallace in *Nature*, vol. x. p. 459. Thus, to quote from the essay: "During the long course of ages, let valleys become converted into estuaries, and then into wider and wider arms of the sea; and still I can well believe that the impulse [originally due to seeking food] which leads the pinioned goose to scramble northward would lead our bird over the trackless waters; and that, by the aid of the unknown power by which many animals (and savage men) can retain a true course, it would safely cross the sea now covering the submerged path of its ancient journey."

The next topic considered is that of instinctive fear. Many facts are given showing the gradual acquisition of such instinctive fear, or hereditary dread, of man, during the period of human observation. These facts led Mr. Darwin to consider the instinct of feigning death, as shown by sundry species of animals, when in the presence of danger. Seeing that 'death is an unknown state to each living creature,' this seemed to him 'a remarkable instinct:' and accordingly he tried a number of experiments upon the subject with insects, which proved that in no one case did the attitude in which the animal 'feigned death' resemble that in which the animal really died; so that the instinct really amounts to nothing else, in the case of insects at all events, than an instinct to remain motionless, and therefore inconspicuous, in the presence of danger. From the facts given with regard to certain vertebrated animals, however, it is doubtful how far this explanation can be applied to them.

A large part of the essay is devoted to 'Nidification and habitation,' with the object of showing, by an accumulation of facts, that the complex instincts of nest-building in birds and of constructing various kinds of habitations by mammals, all probably arose by gradual stages under the directing influence of natural selection.

The essay concludes with a number of 'miscellaneous remarks' on instincts in general. First the variability of instinct is proved by sundry examples; next the fact of double instincts occurring in the same species; after which, "as there is often much difficulty in imagining how an instinct could first have arisen," it is thought "worth while to give a few out of many cases of occasional and curious habits, which cannot be considered as regular instincts, but which might, according to our views, give rise to

¹ From *Nature* of Dec. 6.

such." Finally, cases of special difficulty are dealt with. These may be classified under the following heads: 1. Similar instincts in unallied animals; 2. Dissimilar instincts in allied animals; 3. Instincts apparently detrimental to the species which exhibit them; 4. Instincts performed only once during the lifetime of an animal; 5. Instincts of a trifling or useless character; 6. Special difficulties connected with the instinct of migration; 7. Sundry other instincts presenting more or less difficulty to the theory of natural selection.

The 'conclusion' gives a summary of the general principles which have been set forth by the whole essay. This, therefore, we shall quote *in extenso* :—

"We have in this chapter chiefly considered the instincts of animals under the point of view whether it is possible that they could have been acquired through the means indicated on our theory, or whether, even if the simpler ones could have been thus acquired, others are so complex and wonderful that they must have been specially endowed, and thus overthrow the theory. Bearing in mind the facts given on the acquirement, through the selection of self-originating tricks or modification of instinct, or through training and habit aided in some slight degree by imitation, of hereditary actions and dispositions in our domesticated animals, and their parallelism (subject to having less time) to the instincts of animals in a state of nature; bearing in mind that in a state of nature instincts do certainly vary in some slight degree; bearing in mind how very generally we find in allied but distinct animals a gradation in the more complex instincts, which shows that it is at least possible that a complex instinct might have been acquired by successive steps, and which, moreover, generally indicates, according to our theory, the actual steps by which the instinct has been acquired, inasmuch as we suppose allied instincts to have branched off at different stages of descent from a common ancestor, and therefore to have retained, more or less unaltered, the instincts of the several lineal ancestral forms of any one species, — bearing all this in mind, together with the certainty that instincts are as important to an animal as their generally correlated structures, and that in the struggle for life under changing conditions slight modifications of instinct could hardly fail occasionally to be profitable to individuals, I can see no overwhelming difficulty on our theory. Even in the most marvellous instinct known, that of the cells of the hive-bee, we have seen how a simple instinctive action may lead to results which fill the mind with astonishment.

"Moreover, it seems to me that the very general fact of the gradation of complexity of instincts within the limits of the same group of animals, and likewise the fact of two allied species placed in two distant parts of the world and surrounded by wholly different conditions of life, still having very much in common in their instincts, support our theory of descent, for they are explained by it; whereas, if we look at each instinct as specially endowed, we can only say that it is so. The imperfections and mistakes of instinct on our theory cease to be surprising; indeed, it would

be wonderful that far more numerous and flagrant cases could not be detected, if it were not that a species which has failed to become modified and so far perfected in its instincts that it could continue struggling with the co-inhabitants of the same region, would simply add one more to the myriads which have become extinct.

"It may not be logical, but to my imagination it is far more satisfactory, to look at the young cuckoo ejecting its foster-brothers, ants making slaves, the larvae of the Ichneumonidae feeding within the live bodies of their prey, cats playing with mice, otters and cormorants with living fish, not as instincts specially given by the Creator, but as very small parts of one general law leading to the advancement of all organic bodies, — Multiply, vary; let the strongest live and the weakest die."

DR. GRINEWETZKY'S CROSSING OF NOVAIA ZEMLIA.

ON the $\frac{1}{13}$ November, Dr. Grinewetzky described, before the Geographical society of St. Petersburg, his travels on this island. He first started on foot on the $\frac{5}{17}$ August, with Kriwoskeya and a Samoyede (a few of whom are found near Karmakuly). The weather was beautiful, the thermometer 5° C.; but soon after reaching a mountain with a very extensive view, where they passed the night, they were overtaken by a violent snow-storm, and compelled to return. In April, 1883, the Samoyede Hametz crossed the island to the south-east coast, and found Samoyede *chums* (tents). Hearing of this, Grinewetzky, accompanied by Hametz and another Samoyede, set out in sleds drawn by dogs. They had scarcely any food for the dogs, but were assured they would find plenty, as wild reindeer were abundant. This proved not to be the case; and on the fifth day the poor dogs were near starving, when a large herd of reindeer was met. Many shots were fired without effect, due to the difficulty of seeing distinctly, as the men's eyes were much affected by the reflected sunlight, and, in addition, their hands were benumbed by the cold (—20° to —25° C.). At last two were killed, and the dogs saved. At first a number of very steep parallel ridges, principally of black slate, were encountered. At some places, hard and exceedingly steep snow-drifts had to be avoided by ascending the surrounding hills. Excepting these drifts, there was but little snow, as, if loose, it was swept away by the strong east-south-east wind prevailing. After the watershed between the west and east coasts is passed, the country becomes a low plateau, and the snow softer and rather deep and regular. On the $\frac{30}{12}$ April ^{May}, with the temperature at —27° C., they prepared to return, as they had already proceeded two days farther than was intended, and no chums were in sight; and, although one of the Samoyedes said the chums were only three miles distant, they began the return.

This expedition is important as the first crossing of Novaia Zemlia by civilized man. According to information collected by Tjagin (1878–79), Pakhtus-

sow, Ziwołka, and Moisseew (1832-39), and a few notes by Höfer and Nordenskiöld, and from his own observations, Grinewetzky gives the following sketch of the south island of Novaia Zemlia. It may be divided into three parts. The northern lies between Matotschkin Shar on the north and the Pukowaja River on the south: this part has the highest mountains (four thousand feet), forming isolated groups rather than ranges. The central part, extending to the Karelka and Belushia Rivers, has five or six parallel ridges, running generally north and south; black slate is common; and the watershed is about seventeen miles from the west coast. The southern part is a rather low plateau: the Goose Land (Gusiwaya Zemlia) is included in this part, which is free from snow by the end of June, and in July has a rather rich vegetation, especially on the gently sloping ground.

Dr. Grinewetzky also expressed the opinion that the wild reindeer of the northern island belong to a totally distinct sub-species from those of the southern island.

LOSS OF NITROGEN FROM ARABLE SOILS.

THE renewed attention of agriculturists has of late been drawn to the question of the nitrogen supply of cultivated soils. On the one hand, Schulz, in Germany, claims to have brought about a gain of nitrogen on a sandy soil by means of the cultivation of lupines, and manuring with kainit. On the other hand, Lawes, Gilbert, and Warington,¹ in England, have published results which show that a very considerable annual loss of nitrogen occurs in the drain-water of cultivated fields; and experiments by Dehérain,² in France, show, according to his interpretation of them, an alarming decrease in the total nitrogen of the soil in the course of a few years, and in spite of abundant manuring.

Schulz's experiments have added nothing to our knowledge of the natural supply of nitrogen to the soil, and it is not proposed to consider that topic here. The results of Lawes, Gilbert, and Warington, and of Dehérain, however, have attracted much attention. If they are to be accepted without reserve, they lead to the conclusion that the fertility of our cultivated fields, so far as it depends upon their nitrogen, is being removed in the drainage-water, or in other ways, at a comparatively rapid rate.

The instigation to Lawes, Gilbert, and Warington's experiments was given by the observation, that, in the field-experiments carried on for a series of years at Rothamsted, scarcely a third of the nitrogen of the manure was found in the crop under the most favorable conditions, while, in those cases in which no mineral manures were applied, the deficit was much greater. The most obvious conclusion was, that there must be a great loss of nitrogen in the drainage; and experiments were instituted to test this idea. Their earlier experiments were with three lysimeters. Excavations were made under and around an area of

a thousandth of an acre. The mass of soil thus isolated was supported by perforated iron plates, and surrounded by masonry, thus leaving the soil with its natural structure. The quantity of water percolating through this soil has been determined since 1870; and since May, 1877, its content of nitrates has been also determined. The soil was uncultivated and free from vegetation. Numerous interesting facts are disclosed by these determinations, but that which now interests us chiefly is the quantity of nitrogen found in the drain-water. This amounted, in the average of four years, to 46, 36, and 44 pounds per acre, at depths respectively of 20, 40, and 60 inches.

Subsequently the same experimenters have determined the nitrates in the drainage-waters from their experimental wheat-field, each plot of which is drained by a single lateral at a depth of 24 to 30 inches. Having no means of measuring the drainage, the authors take, as the basis of their calculation of the loss of nitrogen, the amount of drainage-water yielded by the 60-inch deep lysimeter at the same time. On this assumption, the annual loss of nitrogen varied from 15 and 16 pounds per acre, on unmanured plots, to as high as 74 pounds per acre.

It is greatly to be regretted that the authors were not able to measure the drain-water in these experiments; for the method which was adopted to supply the deficiency leaves much to be desired. The soil in the lysimeter was uncultivated and bare of vegetation: that of the wheat-field was cultivated, and bore crops of wheat varying considerably in amount. Both these circumstances affect the amount of drainage-water. Cultivation, especially of a clay soil such as that at Rothamsted, may affect very markedly the ease with which water passes downward through it, the amount of water which it can retain in its interstices, and the rapidity of evaporation from its surface. The growth of vegetation exerts a still greater effect on the movements of water in the soil. It has been shown by numerous observers, that much more water evaporates from a soil covered with vegetation than from a bare soil, and that consequently much less of the rainfall percolates through the soil. The diminution of the drainage-water in this way has also been directly proved by Wollny. Furthermore, the various plots in these experiments carried unequal quantities of vegetation, so that the amount of evaporation due to this source must have been unequal also. It appears, then, in the highest degree improbable, that the quantity of drainage-water actually was the same for each plot as was assumed, and unlikely that it was as great as was assumed. When we add to these considerations the fact, that it is uncertain whether the soil of the lysimeter represented an average of the soil of the field, and, further, that all errors of the lysimeter are multiplied a thousandfold when the results are expressed per acre, we are forced to the conclusion that the figures given for the total amount of drain-water, and consequently those also for the total loss of nitrogen in this way, can be, at best, only approximations, and are most likely too large.

¹ *Journ. roy. agric. soc.*, xvii. and xviii.

² *Annales agronomique*, viii. 321.

Even if we allow them their full value, however, they do not in all cases cover the entire loss of nitrogen. On the basis of the results just described, the authors have calculated the average annual amount of nitrogen in the manure, crop, and drainage of fourteen experimental wheat-plots for a period of thirty years. In seven cases the nitrogen found in crop and drainage is from 14 to 40 pounds per acre less than the amount applied as manure. Analyses of the soil of one plot in 1865, and again in 1881, showed that about a third of this amount was still present in the soil, the latter having gained nitrogen. The authors believe the remainder either to have escaped the drain-tiles, and been carried into the lower strata of the soil, or to have been set free in the gaseous state in the soil.

Dehérain's experiments were intended to determine the total loss of nitrogen by the soil. Three series, of four plots each, were laid out, each plot having an area of one are (equal to about four square rods). The first series bore fodder-maize; the second, potatoes; the third, beets, fodder-maize, and esparcette successively. During the first three years, one plot in each series was unmanured; one received, per hectare, 80,000 kilograms of stable-manure; one, 1,200 kilograms of nitrate of soda; and one, 1,200 kilograms of sulphate of ammonia. During the following four years none of the plots received any manure. At the beginning of the experiments, and at the end of three and seven years respectively, the percentage of nitrogen in the soil was determined. With the aid of these determinations, a balance was struck for each plot between the nitrogen originally present and that added in the manure, on the one hand, and that removed in the crops and remaining in the soil at the close of the experiments, on the other hand. In every case except that of the esparcette, a very great loss of nitrogen was found to have occurred. The following table contains the annual loss of nitrogen from a portion of the plots, reduced to pounds per acre to compare with Lawes, Gilbert, and Warington's results:—

Maize plots.

Manuring.	First period.	Second period.
Stable-manure	— 257	— 118
Nitrate of soda	— 358	— 183
Sulphate of ammonia	— 320	— 132
Nothing	— 338	— 93

Esparcette plots.

Manuring.	First period (roots and maize).	Second period (esparcette).
Stable-manure	— 808	+ 327
Nitrate of soda	— 663	+ 134
Sulphate of ammonia	— 732	+ 135
Nothing	— 567	+ 149

Compared with the losses observed in Rothamsted, some of these figures are enormous, being over nine

times as great as the highest obtained there. When we consider that the soil was calculated to contain, to a depth of 14 inches, only about 7,000 pounds of nitrogen per acre, they seem to show that but a comparatively short time would be required to reduce the supply of nitrogen to the point at which culture ceases to be profitable.

In order to be able to judge of the force of these results, it is desirable, in the first place, to consider somewhat more in detail the method by which they were obtained. At the beginning of the experiments the soil was found to contain .204% of nitrogen; and this was made the basis of the calculation for all the plots. It is highly improbable that this assumption of uniformity among all the plots, as regards nitrogen, is correct; and, when we consider that a difference of .001% corresponds to a difference of about 34 pounds of nitrogen per acre, we are led to question, not only the accuracy of Dehérain's results, but the possibility of discovering small losses of nitrogen by means of soil-analysis. The absolute quantity of nitrogen was calculated on the further assumption that one hectare of soil to a depth of 35 centimetres weighed 3,850,000 kilograms, which, again, involves a possibility of error. Finally, the determination of the quantity of nitrogen removed in the weighed crops rests on assumptions as to the percentage of nitrogen they contained, while it is a well-established fact that considerable variations in this respect occur. Especially does heavy manuring with nitrogenous fertilizers, such as some of these plots received, tend to increase the percentage of nitrogen in the crop.

To recapitulate: Dehérain's conclusions involve three improbable assumptions; viz., perfect uniformity of soil as regards nitrogen, equal weight of soil over equal areas, and a uniform and average percentage of nitrogen in the crops.

On the other hand, all but a few of the plots show a loss of nitrogen; and while, for the reasons just stated, the accuracy of the results is very questionable, it would appear that we must admit some loss of nitrogen to be probable in most of the experiments.

Aside from these considerations, however, there are others which should be borne in mind. During the first three years the manured plots were very heavily manured. Sulphate of ammonia and nitrate of soda were applied at the rate of 1,070 pounds per acre, and stable-manure at the rate of over 35½ tons per acre, — quantities much greater than would be used under any ordinary conditions. Moreover, from the fact that the unmanured plots yielded nearly as large crops at the close of the experiments as at their beginning, we may conclude that the soil was naturally of good quality.

Dehérain's calculations show that but a very small proportion of the nitrogen of the manures was utilized by the crops; and, though the exactitude of his figures may be questioned, the general result is what we should expect. A large excess of available nitrogen was evidently present in the soil. The latter was occupied by crops for only four or five months of the year, at most (except the esparcette plots); and, during the remaining two-thirds of the year, leaching, and other

natural agencies which tend to remove nitrogen from the soil, had undisputed possession of the field. When the manuring was discontinued, the losses of nitrogen, according to Dehérain's results, sank very materially, though still remaining considerable.

From the combined results of both these investigations, it would appear that we may fairly conclude, that, under ordinary conditions of tillage, there is considerable loss of nitrogen from the soil. Lawes, Gilbert, and Warington's experiments show that much nitrogen may escape in the drainage; and, according to their calculations, more nitrogen was removed from six out of thirteen of their experimental plots in crop and drainage, during thirty years, than was supplied in the manure. From Dehérain's experiments we learn that a soil under constant tillage may grow poorer in nitrogen in spite of heavy manuring. In fact, of all the elements of the soil which are required for plant-growth, nitrogen is one of the most mobile. The soil, it is true, has the power of fixing ammonia in insoluble combinations; but both ammonia and organic nitrogen are constantly being converted into nitric acid in every fertile soil, and this compound the soil has no power to retain. Under natural conditions, when the soil is thickly covered with vegetation, this nitric acid is assimilated by the roots as rapidly as it is formed, while the compact state of the soil hinders access of oxygen to the deeper layers, and thus moderates nitrification. This action of plant-roots in arresting nitrates on their way to the lower strata of the soil is shown very plainly in Lawes, Gilbert, and Warington's experiments already cited. While the land carried a crop of wheat, the drain-water contained little or no nitrates, except when an excess of nitrogen had been given in the manure; but as soon as the crop was removed, nitrates made their appearance in the drain-water.

But an untilled soil is not only protected against losses of nitrogen: it is also in condition to retain the nitrogen brought to it in rain, snow, etc. This comes partly in the form of ammonia, which is fixed by the soil, and partly in the form of nitric acid, which is fixed by the vegetation. In this way a soil carrying permanent vegetation may be continually gaining nitrogen. This is indicated by Dehérain's results on the esparcette plots, and, aside from them, is sufficiently evident from the facts, that at some period of the world's history all its nitrogen must have existed in the free state, and that, so far as we know, the combined nitrogen of atmospheric precipitates is the sole natural source of nitrogen to the soil.

Tillage alters this state of things very materially. By breaking up and mellowing the soil, it facilitates the access of oxygen, and increases the rapidity of nitrification. At the same time, the natural vegetation is replaced by one occupying in many cases but a part of the ground, and occupying it for but a portion of the year. Add to this that by diminishing the amount of vegetation we diminish the evaporation of water, and thus leave the soil moister, and at the same time expose it more fully to the sun's rays, thus rendering it warmer, both of which conditions favor nitrification, and we see that cultivation both increases the

flux of nitrogen in the soil, and decreases the means of utilizing it.

The clear recognition of this state of things brings with it the suggestion of at least a partial remedy, which is to keep the soil occupied as fully and as long as possible with growing vegetation. The roots of the living plant lend to the soil an absorptive power for nitrogen compounds, similar to that which it has of itself for other elements of plant-food, and enable it to store up these compounds against future needs. To prevent a loss of nitrogen, we must make use of this power as fully as possible, both in the system of cultivation adopted, and in other ways. After taking off a crop in the early fall, instead of leaving the land bare, let it be sown with some quick-growing crop, e.g., rye, which shall serve solely to store up the nitrogen which would otherwise be lost. In the spring this crop is ploughed under, and furnishes nourishment for the succeeding crop. Such a plan has been adopted here and there with advantage. Its general use would turn largely, of course, on the question of expense. On a virgin soil containing already large reserves of nitrogen, no appreciable benefit might result from it, though even there the *preservation* of the present fertility is worth striving for. But between this condition and the state of relative exhaustion to which the soil of our older states has been reduced, there must be a point where saving nitrogen in this way would be of immediate as well as prospective benefit. The exact methods of applying the principle involved to particular cases it is not the province of this article to discuss. The principle itself, however, is very simple. Keep growing roots present in the soil as long and as extensively as possible to seize upon the nitrogen (and other elements as well) which will otherwise be washed out of the soil, and to store it up in insoluble forms, ready for the needs of future crops.

H. P. ARMSBY.

THE LIFE OF HAMILTON.

Life of Sir William Rowan Hamilton, Knt., LL.D., D.C.L., M.R.I.A., Andrews professor of astronomy in the University of Dublin, and royal astronomer of Ireland, etc.: including selections from his poems, correspondence, and miscellaneous writings. By ROBERT PERCEVAL GRAVES, M.A., sub-dean of the Chapel royal, Dublin. Vol. i. London, Longmans, Green, & Co., 1882. 20+698 p. 8°.

THIS volume, which forms one of the latest issues of the Dublin university press series, has been prepared partly through the assistance furnished by the Board of Trinity college, and published by the provost and senior fellows. Mr. Graves had at first, however, undertaken the biography of Hamilton on his own responsibility, and unassisted in the labor which it involved; and we ought not to pass unremarked his especial fitness for the performance of this arduous task. In the first place, he was unconnected with Hamilton by

any tie of kindred. Both had experienced unbroken friendship from early youth. Hamilton, in his will, had nominated Mr. Graves as his literary executor; and the sons of Hamilton asked him to undertake the task, seconded by the approval of several of the most influential friends of the great mathematician. And, while Mr. Graves has to confess himself to be no mathematician, he combined — what was of greater import — the requisite amount of personal knowledge with the appropriate scientific attainments and freedom from incompatible engagements. In his preface, the author very gracefully says, by way of allusion to his self-distrust in assuming the control of Hamilton's voluminous papers and correspondence, "I gave a reluctant consent, wishing that the memory of my friend had been more fortunate, but at the same time conscious that by me would be devoted to it the warmth of honest affection and admiration, and the desire to be just and truthful."

In recording the successive mathematical discoveries of Hamilton, Mr. Graves does not attempt accurately to appreciate their importance, or to give them their exact place in connection with precedent or subsequent discovery. He has taken pains to secure that the mathematical statements in his work are correct, giving them generally in the *ipsissima verba* of Hamilton himself, and, where in doubt, consulting friends of competent authority. This course begets a desirable confidence in the accuracy of the entire work, — which is, however, taken as a whole, almost purely a literary biography. It is not so much to the credit of Mr. Graves as may at first seem probable, that he leaves the letters of Hamilton almost unaided to tell the story of his life. The contributions from the author's pen are very largely of the nature of disconnected comment, usually upon a subjoined letter: in fact, there is nothing approaching a continuous analysis of the life or work or character of Hamilton, such as we may hope to see in a subsequent volume, and which Mr. Graves, from his evidently keen insight, and thorough acquaintance with the subject of his biography, is of all persons most fully qualified to write. Nor could the book have been otherwise than improved, had he drawn very largely from his own association and personal recollection of Hamilton in the interest of those who never knew him.

The name of William Hamilton has conferred a threefold distinction upon the kingdoms of Great Britain. An early article on the subject of this biography reminds its readers that each isle has its Sir William Hamilton.

The Englishman was noted for his patronage of art, the Scotchman was among the first in philosophy, and the Irishman was among the first in mathematics. And the promise of greatness the young Irishman gave at that early day failed in no sense of entire fulfilment in the development of mature years. Of the three Hamiltons, William Rowan was easily the chief. We recall in this connection what some of his most distinguished contemporaries have said of him. The celebrated Dr. Brinkley, astronomer royal of Ireland (later Bishop of Cloyne, and whose successor in the former office the youthful Hamilton was so soon to be), said of him at the age of eighteen, "This young man, I do not say *will be*, but *is*, the first mathematician of his age." The brilliant and learned Professor Sedgwick, referring publicly to Hamilton in 1833, spoke of him as "a man who possessed within himself powers and talents perhaps never before combined within one philosophical character." Hamilton was born in Dublin, Aug. 4, 1805, and died in the same place, from an attack of gout, Sept. 2, 1865, being then royal astronomer of Ireland. His early life is the story of alarming precocity, not of invention, but of acquisition. Nothing could have seemed more certain to those who knew the boy of half a score than that middle life would easily insure him rank as the chief of linguists. At five he was able to read and translate Latin, Greek, and Hebrew; at eight, he knew Italian and French; and before the age of ten his father wrote of him, "His thirst for the oriental languages is unabated. He is now master of most, indeed of all except the minor and comparatively provincial ones. The Hebrew, Persian, and Arabic are about to be confirmed by the superior and intimate acquaintance with the Sanskrit, in which he is already a proficient. The Chaldee and Syriac he is grounded in, and the Hindoostanee, Malay, Mahratta, Bengali, and others. He is about to commence the Chinese." One of Hamilton's earliest productive efforts was the preparation of a little manuscript book of thirty pages, formally entitled 'A Syriac grammar, in Syriac letters and characters,' etc. (p. 51). He was not as yet twelve years of age; and before another year had passed, his works included (these are the titles given by the boy himself) 'A grammar of the Sanskrit language,' 'An Arabic praxis,' 'An analysis of a passage in Syriac,' and 'A compendious treatise of algebra,' which latter proceeds as far as quadratic equations. Up to this point, Hamilton seems to have had no marked disposition toward scien-

tific studies. He had been fascinated by telescopic views of the planets, and had visited the Royal observatory at Dunsink. Unquestionably one of the most important events in his early career was the meeting of Zerah Colburn. The two had engaged in trials of arithmetical skill when the former was only twelve; but two years later they re-met, Hamilton being "not so much the antagonist as the critic and the investigator of the methods of the gifted computist." That it would be difficult to over-estimate the significance of this occurrence is evident from a letter by Hamilton to his cousin Arthur, in 1822, wherein he says (p. 111), —

"I was amused this morning, looking back on the eagerness with which I began different branches of the mathematics, and how I always thought my present pursuit the *most* interesting. I believe it was seeing Zerah Colburn that first gave me an interest in those things. For a long time afterwards I liked to perform long operations in arithmetic in my mind; extracting the square and cube root, and everything that related to the properties of numbers. It is now a good while since I began Euclid. Do you remember when I used to go to breakfast with you, and we read two or three propositions together every morning? I was then so fond of it, that, when my uncle wished me to learn algebra, he said he was afraid I would not like its uphill work, after the smooth and easy path of geometry. However, I became equally fond of algebra, though I never mastered some parts of the science. Indeed, the resources of algebra have probably not been yet exhausted."

The practical bent of his young mind in scientific matters is interestingly shown by his invention of a telegraphic signal-code, which, for a youth of fifteen, is not a little remarkable. The letters of the alphabet were first arranged in the following scheme (p. 88) : —

	1	2	3	4	5
1	A	B	C	D	F
2	G	E	H	J	K
3	L	M	I	N	P
4	Q	R	S	O	T
5	V	X	Y	Z	U

Twice U = W

Then five readily distinguishable positions of the arms were chosen. Each letter, thus, would be indicated, after the manner of a double-entry table, by its position at the intersection of a horizontal and a vertical column; and the numbers of these intersecting columns, transmitted from one station to the other by

the pre-arranged signals, would thus spell out any desired message. It will be observed that the duplication of any given position of the arms always indicates a vowel. This device for communication at a distance was for a time practically employed by Hamilton and a playmate of his, each being provided with a telescope, so that he could readily discern the successive positions of the arms of the other. His devotion to astronomy had by this time taken firm hold; and Hamilton realized this so fully himself, that he forcibly made in his studies a 'sudden transition to natural philosophy,' excusing himself therefor to his friends by explaining that the "intention was to prevent my giving up too much time to astronomy by diverting my thoughts to another channel: '*atqui emovit veterem mire morbus novus*,' for I am now as deeply engaged in the study of pendulums." In a short paper, at the age of sixteen, he brings science to the assistance of the classics, finding astronomical calculation to help in the decision of a moot-point in the chronology of the Aeneid.

It is most interesting to follow the growth of Hamilton's young mind as his fondness for the mathematics increased, and his devotion to the classics waned. His pre-collegiate letters abound in passages evincing the radical change which was going on, and the solid permanency with which his new favorites had taken possession. A passage from a letter to his sister Eliza, shortly after his entering Trinity college, is cited here as a vigorous illustration of this: —

"One thing only have I to regret in the direction of my studies, that they should be diverted — or rather, rudely forced — by the college course from their natural bent and favorite channel. That bent, you know, is science — science in its most exalted heights, in its most secret recesses. It has so captivated me — so seized on, I may say, my affections — that my attention to classical studies is an effort, and an irksome one."

Immediately on abandoning his absorbing interest in the classics, his work of original research in mathematical optics began. Mr. Graves quotes the title of an "Essay on equations representing systems of right lines in a given plane," etc., — a paper of twenty-one folio pages, to which Hamilton himself had appended the following note: "(This curious old paper, found by me to-day in settling my study, must have been written at least as early as 1822. It contains the germ of my investigations respecting Systems of rays, begun in 1823. W. R. H., February 27, 1834.)"

Hamilton's college career was a most brilliant one. During no small portion of his leisure, he was at work developing the germs of the above-named investigation, which, in the spring of 1827, was presented to the Royal Irish academy, having been expanded into 'A theory of systems of rays.' The first part was published the following year in the fifteenth volume of the academy's transactions. His collegiate course had not been completed, when, less than twenty-two years of age, he was unanimously elected Andrews professor of astronomy in the University of Dublin, and royal astronomer of Ireland, — an extraordinary preference for an undergraduate, who had for competitors men of high standing and eminence in two universities. His appointment under these circumstances involved another exceptional event: by the donor's direction, the professor of astronomy is one of the examiners for Bishop Law's prize, which is yearly bestowed upon the best answerer in the higher mathematics among candidates of junior bachelor standing. The new occupant of the chair of astronomy was, within a few days of his appointment, called upon to take his part in the examination; an undergraduate thus officially examining graduates in the highest branches of mathematics.

In the following autumn, Hamilton met the poet Wordsworth. Their correspondence of years, in terms of close intimacy, is very fully given by Mr. Graves, and forms the richest extra-scientific contribution to this biography. We may appropriately allude, in this connection, to Hamilton's poems, with which a very considerable fraction of this large volume is filled. Wordsworth criticised these effusions very freely, and not a few of them are certainly unworthy of Hamilton's better moments. The subjects chosen for versification, however, show an instinctive correctness in the choice of objects and impressions, which, treated by a poet, would be poetry, but, as dealt with by Hamilton, are in general merely healthy ideas plainly and unpoetically expressed in rhyme or verse. Another friendship of Hamilton's we should not omit to mention, — that of the philosopher Coleridge, whom he met in London shortly before the former's death. Their spirited metaphysical correspondence is a very agreeable feature of the present work.

To the wisdom of the same board of electors which, without doubt, saved Hamilton to science from the church (for he had at one time serious intention of entering that body, and was more than once offered ordination), are due

the thanks of mathematicians perpetually for their prompt recognition of the true sphere of his intellectual activity. The duties of his university chair, as director of the observatory, were in large part uncongenial to him, and his brief career as a practical astronomer was not a successful one. His tastes being almost entirely in the direction of mathematical research, it was ultimately fortunate, that, from the commencement of his practice as an observer, his vigor of constitution was seriously impaired. Near the close of 1830 he writes to Sir John Herschel, "I cannot say much for my diligence in observing, but perhaps may have a better account to give of this department after some time; though among other temptations to indolence, I have that of always suffering in health when I attempt night work in the transit-room." He had constant cold in the head and chest, and was much of his time confined to the house. The proposal was soon made that he should change the professorship of astronomy for that of mathematics; and consulting with his friend, the late Dr. Robinson of Armagh, the latter replied. —

"Your course appears to me so clear that there can be no hesitation. As a mathematician you will probably have no equal in Britain, as an astronomer some superiors; for you certainly have not the *practical* enthusiasm which is essential to make one sustain the uniform progress of observing. I was well aware that you are not very fond of observing; but you know you have that in common with Encke (who *hates* it), Airy, and Pond (now never observing)."

In November, 1831, the university board passed a resolution which more than doubled Hamilton's salary, and completely defined his future relations to the university; giving him entire liberty to pursue, as a first object, his mathematical researches, and thus assuming the responsibility of his continuing as a mathematician rather than an astronomer.

Hamilton's friends were not slow to do themselves the honor of proposing his membership of scientific bodies. Through Sir John Herschel he became a member of the Royal astronomical society at the age of twenty-two; three years later he was introduced to the British association for the advancement of science; Lubbock was ready to insure his election to the Royal society (of which, however, he never became a fellow); and in a letter, in 1832, to his intimate companion, Aubrey de Vere, he says, (p. 610), "A hand has lately been stretched forth to me across the Atlantic; a diploma having been sent, with great pomp of broad-seal, and so forth, to tell me that I have been

elected fellow of the American academy of arts and sciences —

"Ueber l nder und meer reichen sich beide die hand."

A picture of curious interest may be drawn from Mr. Graves's occasional touches, portraying Hamilton as a speaker and lecturer. He "had two voices — one deep, rich, sonorous, rhythmical, and solemn, which flowed forth when he delivered a prelection or a speech, or recited poetry; the other soaring acutely into high regions, when he burst into an explanation, or gave vent to some ebullition of good spirits or cheerful comment." At the meeting of the British association in 1832, at Oxford, his speech returning thanks on behalf of the Royal Irish academy contained "a graceful expression of the feelings stirred in him by his peculiar position as the solitary and youthful representative of Ireland on the occasion." Babbage told him, in congratulation, that "an astronomer had no business to be able to speak so well." We have space for only a word from Mr. Graves's charming sketch of Hamilton as a lecturer (pp. 497-498): —

"When he spoke . . . it was plain to see that he was absorbed by a reverential consideration of the grandeur of astronomy. . . . As he poured out in his sonorous tones his thoughts thus blending poetry and science, he appeared . . . absorbed in awed and delighted contemplation of the truths he had the solemn privilege of enouncing; there was no apparent consciousness of his own personality, he was a worshipper revealing the perfections of the object of his worship; and towards the youthful audience who surrounded him he took the attitude not so much of a superior authority and a teacher as of a worshipper desirous that other intelligent spirits should take fire from the flame of his devotion. . . . In these introductory lectures he was wont to indulge himself in refined and eloquent disquisition, in poetic language, quotation and allusion, in tracing the history of the development of the science, and in marking out the achievements of its great promoters. . . . The subsequent lectures of the course were altogether different in style, being rigorously mathematical and demonstrative. . . . They were delivered with an eager simplicity, in a voice often breaking into a high key, strangely contrasting with the deep roll of his oratorical effusions."

One of Hamilton's grandest achievements was the theoretical discovery of conical refraction; and in the popular history of physics he is chiefly known by this. Its prompt confirmation by Dr. Lloyd, in the laboratory of the Dublin university, tended strongly to heighten the dignity of the discovery. It was characterized in terms of most extravagant applause by the greatest physicists of that day. But Hamilton himself, with the unaffected simplicity of true genius, describes it to Coleridge as a 'subordinate and secondary result.' The discovery had no parallel in the history of exact

science: and, as Mr. Graves appropriately remarks, it is only "to be classed with that prediction of the existence of the planet Neptune which has immortalized the names of Adams and Le Verrier."

Nothing, perhaps, will better exemplify Hamilton's rare elevation of character than the following brief words of his biographer: —

"It is to Hamilton's honour that the impression he made upon young men, his coevals and his juniors, was such as to create in them the warmest affection, admiration, and respect. This arose from his unaffected humility and his cheerful communicativeness, combined with his power to solve most difficulties admitting of solution, his frankness in confessing ignorance, his reverential and profound treatment of all great questions."

In so far as it is possible to know the distinguished Irishman from his letters, — and they are presented in the fullest profusion, — the most commanding feature of his character is the absolute absence of every thing akin to meagreness of build: in other words, a thorough and genuine nobility. Repeated illustrations of this might be cited from his correspondence; and it is the most conspicuous element of the admirable frontispiece which has been autographed from a photograph by Chancellor, Dublin, of a miniature bust executed by Terence Farrell in 1833. We should like to express the hope, that, before the conclusion of his task, Mr. Graves will present a print from the other bust of Hamilton, executed, at the request of Lord Dunraven, by the Dublin sculptor, Kirk; in preparation for which a cast was taken from the head, and which thus, as faithfully representing his cranial development, can hardly fail to possess a permanent value.

It is most irksome to be forced from the contemplation of this great genius; for, with this initial volume of his biography, we have to leave him at the age of twenty-seven, and almost in entire anticipation of his characteristic scientific life. His unique researches in the highest fields of mathematical investigation, his great contributions to the science of dynamics, were yet unmade; and the calculus of quaternions, if at all thought of, had no more taken shape than the vague indefiniteness of a dream. If Mr. Graves has disappointed any of his readers in the execution of his task, they must be few, and among those who were so favored as to have enjoyed the intimate acquaintance of the great mathematician. The successive instalments of this exceedingly valuable biography cannot fail to be watched for with eagerness, and welcomed with enthusiasm, by all whose interests embrace the history and development of the exact sciences.

RECENT PROCEEDINGS OF SCIENTIFIC SOCIETIES.

San Diego natural history society.

Nov. 2. — The following officers were elected: president, Dr. G. W. Barnes; vice-president, Joseph Winchester; recording secretary, E. W. Hendrick; corresponding secretary, Rosa Smith; treasurer, C. J. Fox; librarian, Mrs. Z. R. Cronyn; curator, Dr. D. Cave; directors, D. Cleveland, G. W. Barnes, C. J. Fox, E. W. Morse, J. G. Capron.

The following papers were read. Historical notice of *Pinus Torreyana*; by C. C. Parry. — In the spring of 1850, when connected with the Mexican boundary survey, my attention was first called to a peculiar species of pine growing on the Pacific coast, at the mouth of the Soledad valley, San Diego county, by a casual inquiry from Dr. J. L. LeConte, then staying in San Diego, asking what pine it was, growing near the ocean beach at that locality. Not having any specimens to show, he simply mentioned at the time its dense cones, and its long, stout leaves (five in a sheath). Not long after, an opportunity offered to the writer for a personal investigation, having been ordered by Major W. H. Emory to make a geological examination of the reported coal-deposits on the ocean bluff above Soledad. From the notes and collections there made, a description was drawn up, dedicating this well-marked new species to an honored friend and instructor both of Dr. LeConte and the writer; viz., *Pinus Torreyana*, Parry. Of the few specimens then collected, a single cone and bunch was sent to Dr. Torrey to be figured for the Mexican boundary report. While there, it fell under the notice of some inquisitive botanist, who extracted some of the loose seeds, which were planted, but by some inadvertence were mixed with another three-leaved species. When growing, the two different kinds became confounded, and it was inferred that the present discoverer was mistaken in regarding this species as five-leaved. Prof. Parlatore, the elaborator of *Coniferae in de Candolle's 'Prodromus,'* added to this confusion by ignoring the name first proposed, and substituting that of *Pinus lophosperma*. Subsequently, frequent collectors have visited this locality, bearing away to the remotest portions of the world seed of this pine, which, as far as is known, is exclusively confined to a coast-line of not more than four miles, lying between San Dieguito and about a mile below Soledad, and extending scarcely a mile inland. The bulk of the tree-growth is here mainly confined to a series of high broken cliffs and deeply indented ravines on the bold headlands overlooking the sea south of Soledad valley, and within the corporate limits of the town of San Diego. Here, within a radius of not more than half a mile, this singular species may be seen to the best advantage, clinging to the face of crumbling yellowish sandstone, or shooting up in more graceful forms its scant foliage in the shelter of the deep ravines, bathed with frequent sea-fog. One of the finest specimens seen reaches a height of nearly fifty feet, and shows a trunk eighteen inches in diameter at base.

The chair was instructed to appoint a committee of three (to be named hereafter), to report and act upon such measures as may be deemed best for the preservation of the remnant of the *Pinus Torreyana* at Soledad, treated of in the communication of Dr. Parry.

Additions to our flora and fauna; by C. R. Orcutt. — The writer stated, that, since the last annual meeting, over a dozen discoveries have been made in species of plants indigenous to this section, while many more have been discovered unknown hitherto in California.

Notes upon spiders; by Rosa Smith. — *Zilla rosa*, which I discovered at San Diego less than a year ago, is the commonest orb-weaver in San Francisco and vicinity, spinning its delicate snare on trees, bushes, and fences about the city, at Golden Gate Park, and at the Cliff House. Even inside the walls of the California academy of sciences, I have seen its lovely web, accompanied by silken cocoons of its eggs. This spider is easily known by the free radius in the snare, — 'a good wedge cut out of the pie,' Dr. McCook expresses it, — which is peculiar to the genus *Zilla*. At Aptos, near Santa Cruz, I secured an *Epeira*, and cocoon of eggs and young spiders, which have revealed some curious facts in regard to insect parasites. Of these Dr. McCook writes, "One interesting thing about the *Epeira atrata* cocoon is, that it is strangely infested by parasitic and other enemies, no less than four. There were first a number of small reddish ants alive, probably a species of *Solenopsis*, who no doubt were feeding upon the eggs and *débris*; second, several larvae of *Dermestidae*, probably *Atagenus pellio*. These were creeping into the silky interior at will, though some of them were ensconced within the empty cells of some ichneumon. Next I found alive a very small ichneumon fly. I have never yet seen quite such a 'happy family' within the bounds of a spider's egg-nest. The spiderlings seemed to be contented, and indifferent to the presence of these intruders." A few days later, Dr. McCook sent more information, as follows: "Since writing you, I find from Mr. Cresson that the larger ichneumon is a *Pezomachus*; and the small one, as I conjectured, a chalcid of some sort, which is parasitic upon *Pezomachus*. As *Pezomachus* is parasitic upon spiders' eggs, their presence within the cocoon is thus accounted for. By the way, there is a yet minuter chalcid that is parasitic upon the chalcid, that is parasitic upon the *Pezomachus*, that is parasitic upon the eggs of *Epeira atrata* and other spiders."

Natural science association of Staten Island.

Dec. 8. — Mr. Hollick gave an account of the recent discoveries of fossil leaves at Tottenville. There are three distinct kinds of rock containing these fossils, — a hard red or gray ferruginous sandstone, a soft gray sandstone, and a peculiar conglomerate composed almost wholly of vegetable remains cemented together with what is apparently limonite or sesquioxide of iron. In the soft gray

sandstone the remains are not yet destroyed, but are in the form of carbon or lignite. In the other rocks the vegetable tissue has almost entirely disappeared, and only the impressions remain. The rocks are found in blocks or fragments, none of them greater than a foot square, scattered along the beach, mostly at the base of the bluff, which is composed of drift. From our present knowledge, it is not possible to decide whether they were torn up from an outcrop below high-water mark and cast upon the beach, or washed out from the base of the bluff: they no doubt belong to the cretaceous, although our present proofs are not yet sufficient to state this to a certainty.

A note was read from Dr. Britton of Columbia college, in which he stated that the occurrence of similar fossiliferous sandstones on the beach near Glen Cove, Long Island, and vicinity, had been known for some time. There they are found in precisely the same position as at Tottenville, and are associated with extensive beds of fire-clay, kaoline, etc. The Tottenville station is not immediately on these clays, but they are found near by in several directions, notably at Kreischerville. That the two localities mark outcrops of the same geological formation, and probably approximately of the same strata, is almost certain. The physical structure of the Glen Cove series is exactly parallel to that of certain of the clay beds of Middlesex county, N.J., which are well known to belong to the cretaceous epoch. In the absence of sufficient fossil evidence, we cannot state with absolute certainty that the two deposits are equivalent; but there is little doubt that this will ultimately be proven, and that the New Jersey and Staten Island clays, kaolines, lignites, etc., find another and their most northern outcrop on the north shore of Long Island, at or near Glen Cove.

The exact parallelism between our Staten Island specimens and those from Glen Cove, continued Mr. Hollick, can be seen at a glance: in fact, they would be indistinguishable but for the labels, with the exception of the leafy conglomerate before described, which does not seem to be represented elsewhere; it is possibly peculiar to Staten Island.

In determining the genera and species of fossil plants, we have to depend mostly upon the veining of the leaves, which is not by any means so satisfactory as we could wish. Genera can be determined with comparative accuracy. Thus we have no doubt that one of our Tottenville fossils is a willow, though what particular species, it is impossible to say; another is undoubtedly an evergreen, allied to our juniper or arbovitae. The larger specimens are probably willows, viburnums, and sour gums. There are also a few fragments with parallel veins, — no doubt, belonging to the grasses, — a small fruit or nut, and a piece of what appears to be an equisetum or horse-tail rush. These, with other indistinguishable fragments, complete our list.

Cambridge entomological club.

Dec. 14. — Mrs. A. K. Dimmock showed a collection representing stages of thirty-eight species of insects

which are found upon *Betula alba*, the white birch, which will be given later in *Psyche*.

Mr. G. Dimmock showed the two halves of a split wing of *Attacus cecropia*, in which the two layers of the wing had been separated by the following mode. The wing from a specimen that had never been dried is put first into seventy per cent alcohol, then into absolute alcohol, and from the latter, after a few days' immersion, into turpentine. After remaining a day or two in turpentine, the specimen is plunged suddenly into hot water, when the conversion of the turpentine into vapor between the two layers of the wings so far separates these layers that they can be easily parted and mounted in the usual way as microscopical preparations on a slide. This is an easy way of demonstrating the sac-like nature of the wings of insects.

Dr. H. A. Hagen showed preparations to illustrate organs of undetermined function, found on the larvae of Gomphidae, Libellulidae, and Aeschnidae, but not as yet found on Agrionidae, which he believes to be traces of segmental organs. The organs in question are little cavities or invaginations of the epidermis between the segments, one on each side of the median ventral line, on one, two, or three abdominal segments, according to the family to which the larva belongs.

Ottawa microscopical society.

Dec. 18. — Mr. Henry M. Ami read a paper on the use of the microscope in determining fossils, with especial reference to the Monticuliporidae. Late microscopic investigations proved that the more minute organisms found in our rocks were both deserving and requiring such careful investigations; for geologists had been led into erroneous ideas regarding the particular horizon, and range in geological time, of certain species of these fossils from the mere cursory examination given them. Later paleontologists, pursuing their researches in a more scientific manner, had recourse to thin sections of these Monticuliporidae, or fossil Polyzoa, by means of which the true external and internal structures of the zoarium or skeleton of the genera and species belonging to this family were satisfactorily ascertained.

The work of foundation and means devised by Dr. Nicholson (at one time a professor in one of our Canadian universities) inaugurated a new era in the study of these interesting forms. The mode of procedure in preparing thin sections of these fossils was then considered and explained. The different kinds — tangential, longitudinal, transverse, and axial sections — were described, and illustrations of them exhibited in charcoal drawings of some of the common species found about Ottawa city, — *Prasopora Selwyni* Nicholson, *Batostoma ottawaense* Foord, and *Monotrypella trentonensis* Nicholson; the various points exhibited in these sections — such as the large and smaller tubes; cystoid, curved, and straight diaphragm or floors; the spiniform tubuli, etc. — were then described, showing how minutely and accurately their structures and affinities can by this means be detected.

There was still a rich and wide field open for investigation in the study of the Monticuliporidae; and care should be taken first to ascertain with the new and more scientific means the true relations and affinities of the species described previous to 1881.

Mr. Whiteaves exhibited a choice series of recent Polyzoa for comparison with the fossils described in the paper.

Ottawa field-naturalists' club.

Dec. 20. — Mr. James Fletcher read a paper entitled 'Notes on the Flora ottawaensis, with special reference to the introduced plants,' which was explanatory of the lists of plants hitherto published by the club, and in which the non-indigenous species are not indicated. Mr. Fletcher first defined the district from which the plants had been collected, and which lies within a circle of twelve miles radius. He then noted certain of the more interesting of rare or introduced species, and presented lists tabulating the latter plants under the headings of 'Aggressive species,' 'Species able to perpetuate themselves indefinitely,' 'Species dying out after short periods,' etc. An animated discussion ensued, confined principally to the conditions affecting introduced plants, and the spreading of certain species.

Philosophical society of Washington; Mathematical section.

Dec. 19. — Mr. M. H. Doolittle gave a paper on the rejection of doubtful observations, in which observ-

ing-errors were sharply divided into two classes, — those resulting from blunders in recording, pointing on wrong objects, neglect of essential precautions in instrumental adjustment, etc.; and those resulting from an unusual accumulation of similar elements of error. The latter class, because by their magnitude in one direction they indicate that the remaining observations are in error in the opposite direction, he proposed to call *instructive* errors, and claimed that the larger they were the more instructive, and the greater the necessity of retaining them. In practice, however, the best rule with suspected observations is to reject them when they exceed the limit of error possible to the 'instructive' class, and when they fall within it to assign a weight proportional to the chance that the error belongs to the latter class, and not the former. As the law of distribution of the former class of errors (if any such law exist) is very different from the recognized law of the latter class, these questions cannot be decided by computation with a 'criterion,' but must be left to the judgment.

Prof. A. Hall gave as a general result of the debate of this vexed question by Peirce, Airy, De Morgan, Stone, Glaisher, Chauvenet, Gould, Winlock, and others, that *every one can devise a criterion that suits himself, but it will not please other people*. He strongly opposed using such machinery in the discussion of observations as eliminated the knowledge and judgment of the investigator, and giving to results a fictitious accuracy by any sweeping rejection of discordant data.

INTELLIGENCE FROM AMERICAN SCIENTIFIC STATIONS.

GOVERNMENT ORGANIZATIONS.

Geological survey.

Topographical field-work. — Mr. H. M. Wilson, in charge of one of the topographical parties in Prof. A. H. Thompson's Wingate division, surveyed, during the season of 1883, about ten thousand square miles in north-western New Mexico and north-eastern Arizona. The area covered by his work lies between parallels of latitude 36° and 37°, and extends from meridian 109° to 111°. He also worked some smaller detached areas outside of the limits thus indicated. This region has hitherto remained a *terra incognita*, partly on account of its aridity and barren condition, and partly on account of the difficulty of traversing it. So little has been known of it, that within the area surveyed by Mr. Wilson a small mountain range has been indicated as occupying two places on the same map. On the engineer's map of 1879 it is called Calabesa Mountains in the northern place, and Squash Mountains in the southern; and, on the land-office map for 1882, both are indicated without names. Mr. Wilson's work proves that they are one and the same, occupying a position very close to that assigned to the Squash Mountains.

On the 11th of September Mr. Wilson and one of his men made the ascent of Navajo Mountain (called

by the Indians Nat-sis-aú), and they are probably the first white men who have ever stood upon its summit. Navajo Mountain lies on or near the line between Utah and Arizona, and is a dome-shaped mass rising about four thousand feet above the general level of the surrounding country, and sixty-five hundred feet above the beds of the San Juan and Colorado rivers, which are close to its base, the former on the north, and the latter on the west. Its elevation above sea-level is ten thousand four hundred feet. It slopes abruptly, especially on the east, to a plateau of six to seven thousand feet, which extends south-eastward for fifteen or twenty miles to the cañon where Mr. Wilson left his pack-train in camp. This was on a trail that leads to Fort Defiance, *via* the north side of the Mesa de la Vaca and the valley of the Rio de Chelly. Another trail leads southward to Mo-eu-kap-i (a Mormon settlement) and to Oraybe and the other Moqui villages. From a point a few miles south of the Navajo Mountain, a third trail leads westward to Lee's Ferry, on the Colorado River. Mr. Wilson thinks there is also a trail leading to the mountain from the north-west. He says all the trails in this section are exceedingly rough and difficult to travel, on account of the numerous cañons, of five hundred to a thousand feet in depth, which are cut into the red sandstones (triassic?) that form the

surface-rock of the country. On the summit of the mountain, which is about a mile in length, a brownish sandstone occurs, which may possibly be Jurassic or even cretaceous; but all the rocks are probably referable to the Jura-trias, with the exception of some dark igneous rocks which occur as dikes on the slopes of the mountain. Within about a thousand feet of the summit is a spring of good water, where there is a good camping-place. The slopes are timbered; scrubby firs and balsams occurring on the top, with scrub-oaks below, and tall pines still lower down. Among the latter are many beautiful parks. The plateau-level surrounding the mountain is well covered with fine tall grass, over which are scattered patches of piñon pines and small areas of bare red sandstone.

In the walls of a short cañon on the east side of the mountain, passed through by Mr. Wilson in his ascent, ruins of cliff or cave dwellings were seen in a cave or hollow in the rocks about five hundred feet above the bottom, and a hundred feet below the top.

Ascent of Mount Shasta.—Mr. Clarence King, in his 'Mountaineering in the Sierra Nevada,' says, "There is no reason why any one of sound wind and limbs should not, after a little mountaineering practice, be able to make the Shasta climb. There is nowhere the shadow of danger, and never a real piece of mountain climbing,—climbing, I mean, with hands and feet,—no scaling of walls, or labor involving other qualities than simple muscular endurance."

Mr. Gilbert Thompson, who, during the past summer and fall, spent about two months in topographic work on the slopes and summit of Mount Shasta, indorses this statement of Mr. King, and would add that there is no reason why a train of pack-mules may not be taken to the top of the peak. Mr. Thompson and one of his packers (Thomas Watson), on Sept. 10, 1883, tied their riding-mules to the iron signal-post which marks the extreme summit of the cone, and are the first who have ever taken riding-animals to the top of Mount Shasta. On Oct. 12, 1883, the pack-train was taken to an altitude of 13,000 feet, and would have been taken to the top had not the early snows prevented. Another season, however, Mr. Thompson expects to camp with his entire train upon the summit of Mount Shasta. From one of his camps, at an elevation of 7,400 feet, it required seven hours to go to the top with the riding-animals, while one member of the party, starting from the same camp on foot (taking, of course, a more direct route), reached the summit after a climb of six hours. It took two hours to get back to this camp, and three-quarters of an hour sufficed for the return to the camp which was located at the elevation of 13,000 feet. Mr. King and his party in September, 1870, made the ascent from the north-west. The first day they left their riding-animals at an elevation of about 10,000 feet, and climbed as far as the crater on the north-western spur, which point they reached about half-past one o'clock in the afternoon. They spent the night here, and on the following day, after a climb of four hours and a half, reached the summit. Mr. Thompson's ascent, mentioned above, was along a spur that ex-

tends toward the south-east. Up this spur he says there is a natural trail, only 500 or 600 feet of which will require any work to make it perfectly safe for mules or horses with packs. The route described by Mr. King, and the one *via* a south-western spur, are the routes usually followed by those who make the ascent from Strawberry valley, on the west side of the mountain. One member of Mr. Thompson's party climbed the mountain also from the east, which makes, altogether, four different routes by which it has been ascended. Mr. Thompson says there are two other possible ways by which the mountain can be climbed. These are on the north-east side. He reports, also, that there are seven glaciers located on the north and east slopes of Mount Shasta. Those on the north and north-east are connected at their heads. A north-west and south-east line would divide the glacier-bearing side of the mountain from the non-glacier-bearing half. However, some of the fields of snow and ice on the west side have considerable resemblance to glaciers, and may eventually be so determined.

Mr. Thompson suggests that Mount Shasta would be the best point in this part of the west for a permanent high meteorological station like those located on the summits of Mount Washington and Pike's Peak. Among the several reasons for this opinion, he mentions its accessibility, and the presence of hot-springs, which might be utilized in heating such a station, but more especially the fact that it is an isolated peak, rising high above the surrounding low country, and free, therefore, from the disturbing meteorological conditions induced by the presence of contiguous mountain ranges. Mount Shasta, Mr. Thompson says, does not belong to the Sierra Nevada nor to the Cascade Range, but stands alone.

During the season a line of levels was begun at Berryville, where connection was made with the railroad level, and carried some distance up the mountain. Next year this line will probably be carried to the summit of the peak.

NOTES AND NEWS.

THE Society of naturalists of the eastern United States, whose organization and aims were described in *Science* last spring, held a very successful and interesting meeting at Columbia college, New York, on Thursday and Friday of last week. The attendance was very large, and included many distinguished men. The membership has grown very rapidly, and now includes a large majority of the leading professional naturalists of the eastern states. The papers presented were of a high character, and many of them provoked a discussion such as is rarely heard in any scientific body; for seldom are so many men, devoted to one branch of pure science, gathered together. The communications, almost without exception, referred to problems of practical interest, and dealt especially with methods and the organization of scientific work, and also with methods of teaching.

Upon methods were read several papers, — Pro-

fessor James Hall, On a method of preparing rock-sections; Prof. B. G. Wilder, On the preservation of hollow organs, particularly the heart and brain; Prof. S. H. Gage, On the uses of peroxide of hydrogen in preparing skeletons; Dr. George Dimmock, On the uses of carminic acid in microscopical work; Mr. J. H. Emerton, On models of gigantic cephalopods; Dr. E. B. Wilson, On methods of section-cutting. Upon organization we may note Professor Wilder's paper, On the arrangement of a museum of vertebrates, and Professor Cope's, On academies of science in America, etc. Among the educational communications were those of Dr. Wadsworth, Upon methods of teaching in petrography and mineralogy, and Professor Bickmore's, Upon methods of teaching employed at the American museum, etc. The meeting surpassed in interest all expectations, and assures the future standing and prominence of the society, which, although so young, is yet through its membership so strong. The following officers were elected for the ensuing year: president, Professor Alpheus Hyatt; vice-presidents, Profs. H. N. Martin and A. S. Packard, jun.; secretary, Dr. Charles S. Minot; treasurer, Professor William B. Scott; members at large of the executive committee, Profs. H. C. Lewis and Lester F. Ward.

—The Swiss earthquake of 1881 had its centre of intensity in Berne and nearest vicinity, and is one of the best observed as to its extent and details. The geologic formation of the Swiss plateau, where the motions were most intensely felt, consists of a tertiary sandstone of unknown depth, called molasse, while the limestone rocks of the northern Alpine belt and of the Jura ridge were scarcely touched by it. Prof. Dr. A. Forster, the director of the telluric observatory at Berne, has collected a large amount of well-ascertained details on this earthquake, and published it in an interesting quarto memoir of twenty-nine pages, — 'Das erdbeben der schweizerischen hochebene vom 27 Januar 1881 (Berner beben),' Berne, *B. F. Haller*. The scientific results obtained by him may be summed up as follows: the principal shock occurred on Jan. 27, in the afternoon, at 2h. 19m. 53sec., and was preceded and followed by light oscillations of the soil. It took place at a coincidence of the perihelion with the perigee, the new moon being two days and a half later. There were no disturbances of terrestrial magnetism noticed for several days before and after; but a long period of frost had just given way to a sudden thaw, and the upper culmination of the moon had occurred five hours before. The whole area of seismic motion, with its longitudinal axis of two hundred and sixty kilometres, experienced the shock at one and the same astronomic time. There was no central shock, for the dislocation following the shock took place simultaneously upon the whole line. In the majority of places, villages, etc., it consisted of a brief, succussory shaking, followed immediately by a few lateral and less energetic oscillations, all of them possessing a direction running approximately from east to west. The mean duration was but three to four seconds, the intensity varying from three to eight degrees of the Swiss-

Italian seismic scale. Noises usually connected with the heavier earthquakes were heard by most observers who happened to be outdoors; they preceded the shock or were synchronous with it, and none were heard after the shock. South of Martigny (Valais) and north of Mulhouse (Alsace) no disturbance was noticed; though numerous oscillations had occurred one and two weeks before, in southern Germany, Piedmont, and Lombardy. Compare A. Heim, on 'Swiss earthquakes in 1881,' published in the *Annuaire* of the telluric observatory of Berne (1881).

—Messrs. Cassino & Co. and Estes & Lauriat, of Boston, have issued a prospectus of the 'Standard natural history,' — a work in six volumes, imperial octavo, fully illustrated, and under the editorship of Dr. Elliott Coues and Mr. J. S. Kingsley. The staff of writers announced consists of forty-two names, including the larger part of our best-known authors, and all are men of repute. The first volume, on the lower vertebrates, will be by W. K. Brooks, S. F. Clarke, J. W. Fewkes, A. Hyatt, C. S. Minot, A. S. Packard, and others; the second, on the arthropods, by E. A. Birge, J. H. Comstock, A. J. Cook, J. H. Emerton, G. H. Horn, J. S. Kingsley, A. S. Packard, C. V. Riley, P. R. Uhler, etc.; the third, on the lower vertebrates, by E. D. Cope, T. Gill, S. Garman, D. S. Jordan, etc.; the fourth, on birds, by Dr. Elliott Coues alone; the fifth, on mammals, by E. D. Cope, E. Coues, T. Gill, S. Lockwood, G. Macloskie, R. R. Wright, and others; and the sixth, on the races of man, by C. C. Abbott, L. Carr, W. H. Dall, F. W. Putnam, and S. Salisbury. The work will be published in about sixty serial parts, of forty-eight pages each. The numbers already issued leave nothing to be desired in typography, good taste, and excellence in illustration; and we heartily wish so serious a venture every success.

—A new work on the "Theory of deflections and of latitudes and departures, with special applications to curvilinear surveys for alignments of railway-tracks" is in press by Van Nostrand. The author, Isaac W. Smith, is an engineer for some time connected with the construction bureau of the Northern Pacific railroad.

—'The legends of the Panjab,' in monthly numbers from August, 1883, by Capt. R. C. Temple, Bengal staff corps, records, in a form useful to investigators, the stories and legends preserved in the memories of the wandering bards of the Panjab. The legends are given in original in the Roman character, exactly as they were taken down from the lips of the narrators, with translations. The work is being published at the Education society's press, Bombay, and by Trübner & Co., Ludgate Hill, London.

—'The Nautical almanac and astronomical ephemeris for the meridian of the Royal observatory at Greenwich' for the year 1887, commonly known as the British nautical almanac, was published in London late in November. According to *Nature*, the sales of this publication for the last five years have exceeded 15,500 annually.